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ORIGINAL ARTICLES.

ON A CASE OF MYXŒDEMA.

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(*Read before the Medico-Chirurgical Society of Glasgow, 2nd Nov., 1883.*)

Not more than ten years have elapsed since the remarkable disease, of which I have the opportunity of showing you an example to-night, was first recognised and described by Sir William Gull; although no doubt, as Charcot remarks, it must have existed long previously. During the last few years it has become widely known, and has attracted a large amount of attention—a result chiefly due to the investigations of Dr. Ord, of St. Thomas's Hospital, whose descriptions of the disease in its various stages, and whose account of the *post mortem* appearances in two fatal cases, have thrown much light on the subject, and greatly enhanced the interest it has excited. The disease is slow and progressive, and is characterised clinically by a gradual increase of bulk, with a general swelling of the integument, and a peculiar physiognomy; while along with this condition there supervene slowness of speech and of movement, torpor of thought, subnormal temperature, and diminution of the ingesta and excretions. Nearly all the functions of the body seem to suffer more or less, so that Charcot's comparison of the patients to hibernating animals is not altogether inappropriate. But while the general characteristics of the affection may be thus briefly summed up, in detail it presents many minor peculiarities to our notice, most of which will come out in the account of the case I have to show you; so that instead of enumerating these beforehand I shall best avoid repetition by at once proceeding to narrate the case before us. Let me only premise that in doing so my language is almost wholly borrowed from the excellent observers already mentioned.

The patient is a female of 56 years of age. Her father was for a long time a habitual drunkard, who died at the age of 63, of insanity and epilepsy, apparently the result of his intemperate habits, as his parents, as well as his brothers and sisters, were all healthy and long-lived. Her mother died at 62, and was said to be asthmatic; all her relatives on the mother's side were very healthy. The patient is now in the second year of her second widowhood, having been first married at 24. In the preceding year she had a severe attack of rheumatic fever. She has had six children, all daughters; two by her first and four by her second husband. Two of these—the second and fifth—were still-born; the remaining four survive and are quite well. The first still-born child was putrid, and while pregnant with it she suffered from sore throat, with hoarseness and headaches, for a month or two, while a red rash appeared on her skin. Her husband was similarly affected, and confessed to having contracted venereal disease after the birth of their first child. He died of phthisis shortly afterwards. She married again about eight years afterwards, at the age of 34. Her second husband was of drunken habits, and subjected her to much ill usage, so that she has had much grief and anxiety. During her fourth pregnancy her health failed; in particular her left elbow and forearm, as far as the wrist, became red, swollen, and painful, and remained so for a long time. It swelled even more after delivery, but never suppurated. It did not subside altogether for two years, and thus it continued into her next pregnancy, during which she states that her skin became yellow, and her whole body, but especially the abdomen and legs, became greatly swollen. She says that the swelling of the legs pitted on pressure. About the same time the right ear and cheek, and the corresponding side of the neck, became the seat of the same painful swelling as had previously attacked the left elbow, while an eruption of blebs, or bullæ, broke out on the affected parts. These appeared first on the cheek and neck, but by and bye on numerous other parts of the body. They came out in successive crops of three or four at a time, and were filled at first with a clear fluid which in eight days became yellowish; and they finally formed scabs which have left numerous opaque white hard cicatrices on that side of the face and neck. The individual blebs were about the size of halfpence, but of irregular and various forms. The cicatrices have for the most part coalesced, forming a very irregular patch, which contrasts strongly with the surrounding waxy integument. The successive crops continued for nearly two years, and the patient felt better when they were "out." This pregnancy (the fifth) resulted in still-birth, apparently from breech presentation. The swelling of the legs and abdomen which had attended it disappeared, but returned during her next and last pregnancy, at the age of 39. After this she again became thin and supple, and continued so till menstruation ceased, which happened when she was 42. Almost simultaneously with the menopause, she gradually swelled up, so much so that in a few months she imagined she was again pregnant. The swelling, however, affected her whole body, and she at the same time suffered from headaches and giddiness, and became dull, lethargic, and slow in her movements. She, in short, fell into the condition in which you now see her. The swelling seemed to attain its maximum in about a year, remained stationary for the next eleven years, and has diminished considerably within the last two. She has thus been the subject of the affection for at least fourteen years.

On examination, the most cursory glance at the patient is sufficient to convince any one of the distinctive character of the disease from which she suffers; and its striking physiognomy, in particular, may well excite our wonder that it was so long in being recognised. The swelling of the skin of the exposed parts—the hands and face—is at once evident. The former are thick and clumsy; while on comparing the face with this photograph of the patient at the age of 26, you see that it has lost its oval outline and become round; in some cases, as remarked by Sir W. Gull, the face may be so broadened as even to become oval transversely. The alæ nasi are thick, and the nose flattened; the eyelids swollen and hanging in loose folds, while thick folds of skin depend from the lower jaw; even the ears are swollen and pulpy. The tongue is thick and broad, as also the lips; and the lower lip hangs down in the manner characteristic of the disease. It may be remarked, however, that the patient was always thick-lipped, as is evident from her photograph at the age of 26, and a sister who is now in the best of health has also the same peculiarity in a marked form. The œdema is equally pronounced on the chest and abdomen; in the latter situation the swollen, pulpy integuments may be grasped in handfuls. The outline of the clavicle is concealed by the cutaneous thickening, and the swollen masses just above it, at the base of the posterior triangle of the neck, so generally observed in these cases, are tolerably well marked. The thyroid gland is always much atrophied, but owing to the state of the skin about the neck, this is not always easy to ascertain during life. The œdema is not so great in the lower extremities in this patient. The œdematous parts nowhere pit on pressure. The skin is rugose, and freely movable on the subjacent parts, a peculiarity, as pointed out by Dr. Ord, of an advanced stage of the disease. Patient says she was much more swollen two years ago than now, and she can use her hands and arms more smartly now than she could do then, and also walk faster, though fatigue is as readily induced as ever. While the skin is soft and fine, the epidermis is dry and rough, and there is copious shedding of epithelial scales. This harshness diminishes considerably with frequent washing. She sweats very little. The hair of the scalp is scanty, and it has nearly altogether disappeared from the axillæ and pubes. The eyelashes are thin and shortened to stumps; so also with the eyebrows, which are raised till they form quite an acute angle at their outer part, forming not the least noticeable feature of the patient's physiognomy. The nails, like the hair, are brittle, and trouble her by breaking across frequently; her teeth have mostly fallen out. The complexion is yellow and waxy, but the skin is, perhaps, not so translucent as it is generally described in myxœdema—a result probably due to the advanced stage of the disease. It was formerly, she states, much more "shiny" and glossy, and she even dusted it with flour and other substances to disguise it. On the forearms and elsewhere there are some perfectly smooth, pale parts, differing both from the hard cicatrices and the general waxy integument. There is a small circumscribed pink patch on each cheek, formed by dilated blood-vessels, which appear like fine streaks radiating from a centre. The lips are anæmic. It may be mentioned that there is a small mole on the left arm, and two on the right leg; they are acquired and not congenital. Dr. Dyce Duckworth has called attention to these, as occurring in a considerable number of cases.

The peculiarities of the patient's appearance become more marked in speaking and in expressing various emotions than when the features are in repose. There is a stiffness and want of play in the muscular movements, which renders the face expressionless and mask-like. Very slight exercise of the muscles, too, soon causes fatigue; the eyes are but slightly opened; the raising of the upper lid entails evident effort; and the elevation of the eyebrows seems to depend on the occipito-frontalis being habitually called to the assistance of the levator palpebræ. She is frequently obliged to shut her eyes from sheer fatigue. Her speech is slow, deliberate, and monotonous, with nasal intonation. In speaking, the mouth opens but slightly from above downwards, though she can open it wide enough when asked simply to gape. She complains of something contracting or "catching" the upper lip and depriving her of freedom in speaking. Dr. Ord remarks that speaking in these patients is often preceded by the act of swallowing, apparently to set the soft palate in motion. This is not very noticeable in this patient, but she is frequently interrupted, especially if she speaks too much, by acts of swallowing, accompanied with a choking sensation, to relieve which she feels constrained to grasp and rub the neck over the larynx, as if to stimulate the torpid muscles. The saliva does not dribble from the mouth, as often happens, but she may be observed to draw it in with inspiration to prevent this. When she opens the mouth more widely than usual, as in laughing, the tongue may be observed to move in a peculiar rolling, sluggish manner. In actual swallowing, as of her food, she has often difficulty, and has sometimes been nearly choked. She holds her head quite straight; it does not droop forwards on the chest, as in many other cases. Her gait is slow and cautious, as if studying to balance herself at every step, but she does not stagger much. She has never fallen but once, but she seldom walks without support from a wall or other object. Dr. Ord points out that there is no paralysis nor locomotor ataxy in myxœdema, but apparently a want of harmony between flexors and extensors, producing pendulum-like vibrations by the efforts at adjustment. The patient frequently falls, the flexors ceasing to act before the extensors have begun. This patient can use her hands tolerably well, and can pick up a needle or a small coin from the floor.

Her mental faculties have scarcely suffered so much as her muscular powers. Her memory is good; she is fairly intelligent, and often wakens up to a lively interest in conversation, and heartily enjoys a laugh. Her laugh is peculiar, and almost noiseless. She is said to be subject to occasional, but transient, and not very violent, fits of temper. She is sleepless at night, but often somnolent during the day, and cannot resist a frequent tendency to fall into a doze. She has no bad dreams. She has frequent but not very severe frontal headaches, and sometimes vertigo and tinnitus. Muscular efforts which bring on the gulping, choking sensation already mentioned, provoke at the same time the feeling of giddiness. This sensation of choking she attributes to "wind," which, she says, sometimes explodes with great force, when the giddiness is relieved, and the contracted upper lip relaxes. The special senses and the sense of touch are unimpaired, and the sensibility to painful impressions is acute. The limit of confusion in the discrimination of tactile impressions, however, is considerably wider than the normal; she cannot tell whether we touch her at one or more points simultaneously, unless they are widely apart, and the response to impressions is always slow. She cannot look at objects higher than her head for any length of time; the attempt makes her giddy and inclined to fall backwards.

The tongue is furred; she takes very little food and drink, and complains of foul eructations; she is habitually costive, and subject to hæmorrhoids. The pulse has ranged between 80 and 92, of fair strength, but not, as I should judge, very resistant; there is an auricular-systolic murmur at the heart, loudest at the apex, and the second sound is accentuated over the aortic area. The pulse is often much slower in myxœdema than in this case. Respiration generally 18, extremely shallow. There is no dyspnœa; on the contrary, there seems to be but a limited demand for oxygen, and there can be no doubt that the excretion of carbonic acid is considerably lessened in this and other cases of myxœdema, but no observations seem to have been made on this point. Breath sounds are normal, but feeble; expiration is almost inaudible in ordinary breathing. She can take a long, deep breath when desired to do so, and after a few deep inspirations she can hold her breath for sixty seconds without inconvenience. Dr. T. Oliver, in a clinical lecture on myxœdema, has called attention to a pause between inspiration and expiration equal in duration to the inspiratory act itself. This he has observed in two cases, but I can hardly say such is the case with this patient; I would rather say that expiration, after a long inspiration, is extremely slow throughout.

The temperature in the axilla when first taken was 97·4°, and it has ranged from 96·4° to 98·8°. She is often cold, sometimes to the point of shivering, and her hands are cold to the touch. The average quantity of urine passed has been 25 ounces in the twenty-four hours. It is rather pale, feebly acid, and deposits a good deal of vesical mucus. I have never detected in it a trace of albumen or sugar. The urea has been several times estimated by the ureameter, and amounted to an average of 170-180 grains daily. The absolute amount of uric acid and phosphates excreted seemed to correspond pretty closely with the diminution in the excretion of the urine itself, judging from some rough comparisons with specimens of normal urine.

A history of syphilis in myxœdema, as in the present instance, is quite exceptional, and apparently the former has no causal relationship to the latter disease. The great majority of reported cases (which have not been numerous, however, not many more than twenty having been hitherto published), have occurred in females, generally commencing about the climacteric, and frequently after confinements (Dr. Ord). In nearly all there is the same history of privation, grief, and anxiety, so that in every respect this patient is a very typical example of the disease from which she has suffered fourteen years. A few cases have also been reported in males. It seems in nearly every instance to be due to some exhausting or depressing influence, although why this should produce such a result in this small number of cases is quite inexplicable. One case has been reported after a wound of the neck; another after severe *post partum* hæmorrhage. A few cases occurring in early life have been published; Charcot showed one such case, but would not decide whether it was a case of late development of cretinism, or early supervention of myxœdema, although there was no endemic or hereditary influence in the case likely to give rise to the former disease.

Dr. Mahomed has boldly started and advocated the view that myxœdema is a form of chronic Bright's disease without albuminuria, but this, I think, however ably supported, we can hardly accept. A single case is almost enough to convince any one that myxœdema is a distinct and separate clinical entity. There is a very strong family likeness, too, between the individual cases; greater than we generally find in other diseases. It is true that the fatal cases hitherto recorded have terminated with albuminuria and coma, and that the kidneys have presented an appearance resembling that of chronic interstitial nephritis. But, as Dr. Ord insists, albuminuria seldom appears earlier than the last few months of the disease, and the patient may be entirely free from it for six or even ten years, although presenting all the features of myxœdema in a pronounced form. The present patient, as already mentioned, has been the subject of the affection for fourteen years, and her urine is still quite free from albumen. For the distinction between myxœdema and scleroderma, and the analogies of the former with cretinism, I may refer you to the papers of Sir W. Gull and Dr. Ord.

What is the morbid anatomy of this singular affection? In the answer to this question centres, perhaps, the chief interest of the subject. From a *post mortem* examination of two fatal cases Dr. Ord found a great increase of the connective tissue throughout the body; the nuclear elements were larger, and more abundant than usual, the fibrillæ swollen, and separated from each other by a gelatinous material; the state of the tissue reminding him of that of the umbilical cord, and suggesting the idea of a retrograde degeneration. This was best seen in the skin, in the coats of arteries, and in the glandular organs. This tissue contained a large amount of mucin; portions of skin yielded to chemical investigation fifty or even a hundred times more mucin than healthy integument. This hypertrophy was well marked not only in the skin, but also in the heart, liver, kidneys, thyroid gland, larynx, and other parts. In the skin this gelatinous deposit formed a pad around the touch corpuscles and the terminations of the nerves. In the *post mortem* examination of his second case, though not in his first, he found this increase of connective tissue around the blood-vessels of the brain and spinal cord, and in the central canal of the latter, but he found no evidence of destruction of the nerve elements themselves, nor anything which could strictly be called sclerosis. This increase of mucin induced Dr. Ord to propose the appropriate term "myxœdema" to designate the affection, and the name has met with universal acceptance. Charcot has referred to it as the "cachexie pachydermique," and claims to have recognised it independently, although not the first to publish on the subject. On becoming aware of Dr. Ord's previous investigations, however, he adopted the name of myxœdema, although some French writers still speak of the disease as the cachexie pachydermique.

In this overgrowth of connective tissue we have an explanation of the œdema observed during life, while the ultimate encroachment of the myxosis on vessels and on the secreting elements of glandular structures produces, in Dr. Ord's view, the train of symptoms leading to the fatal termination. In the kidneys, for instance, this thickening, growing inwards from the capsules of the Malpighian bodies encroaches in various degrees up to obliteration on the contained glomeruli of capillaries; hence the albuminuria and coma which supervene towards the close. A full account of Dr. Ord's first *post mortem* examination, with drawings of the microscopic appearances, will be found in the *Medico-Chirurgical Society's Transactions* for 1878. In a fatal case, which was successively under the care of Dr. Mahomed and Dr. Goodhart, it may be mentioned that no excess of mucin was found after death, although the clinical symptoms during life were those of myxœdema. There was only found amongst the muscular fibres a certain amount of a jelly-like material.

Dr. Ord attributes the dull mental state and the slowness of speech and movement to the condition of the periphery. The padding of the touch corpuscles and the cutaneous terminations of the nerves with this mucoid material slows the reception of impressions; and Dr. Ord supposes that the lethargic condition results from the brain being thus deprived of its normal amount of stimulation. This view has excited much attention, and has been warmly criticised in some quarters. Dr. Ord adduces some weighty analogical considerations in support of it, as the effect of padding the ears with cotton, the effect of suppression of the cutaneous excretions, or of a dry, sluggish state of the skin, and of varnishing the skin in animals. But although the peripheral condition may be adequate to account for the phenomena in question in myxœdema, it cannot be considered proved that these altogether depend on it, and that they are not the result of the general cachexia from changes going on elsewhere. Nevertheless, I have been much interested to hear how emphatically my patient states that she becomes livelier and generally improved by friction of the skin, as if she were thus brought into more immediate relationship with the external world. At all events, Dr. Ord, in one of his latest communications on the subject, is still convinced that padding of the peripheral terminations of the nerves is an essential factor in the disease.

The march of the disease is slow and progressive, and various methods of treatment have been tried, but Dr. Ord believes that none do any good. Various cases have been reported as improving under treatment, but the condition of the patient may vary from time to time, and in some cases improvement appeared to result for a time, but was not maintained. One such case, attended with slight albuminuria, is recorded by Dr. Mahomed, which seemed to improve under nitro-glycerine, given to diminish arterial tension; but the improvement was only temporary. Charcot showed a case which had so much improved under the prolonged use of milk diet that he could not show the features of the disease in so marked a form as he wished. At Dr. Ord's suggestion, tincture of jaborandi was used in one of Dr. Oliver's cases. The latter also injected pilocarpine subcutaneously; perspiration did not follow till the dose was increased to half a grain, but no improvement of the symptoms followed. Iodide of potassium, strychnine, phosphorus, and iron have been tried. As I have already observed, my patient has apparently improved much within the last two years, and this has been quite independent of treatment and in the midst of the poorest surroundings.

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CASE OF SCARLET FEVER, FOLLOWED BY COMA AND CONVULSIONS TREATED BY PILOCARPINE.*

By FRANK SHEARAR, M.B., Paisley.

G. P., æt. 8 years, first showed symptoms of scarlet fever on the 27th January, 1883. The attack was a mild one, and after a nine days' illness, interrupted only by an evanescent albuminuria, he seemed convalescent.

* The case occurred in the practice of Dr. Fraser, Paisley, who has asked me to report it.

On the 16th day, however, there was a recurrence of the albuminuria, together with slight œdema of the face and legs. On the 20th day there was slight hæmaturia, but still the patient remained, to all appearance, well. On the 26th day a somewhat livid flush, which the nurse had noticed coming and going for two or three days, became marked, and at 8 P.M. it was noted that the pulse was slightly irregular and only 72 per minute, and that the patient had become markedly lethargic. At 11·30 P.M. the pulse was 140 per minute, and the lethargy had deepened into coma. In five minutes the patient was attacked by convulsions, which were general over face and limbs; they were not violent, and were more of a clonic than a tonic nature, and interfered very little with the breathing. He was immediately placed in a hot pack, and, when the fits had already lasted for ten minutes, what seemed a fair amount of blood was taken from the arm, but without effect on the fits and with very little change in the tension of the pulse. Meanwhile, the convulsions were kept under by means of chloroform, which was not, however, pushed far enough to stop them. After a further lapse of 45 minutes, the fits continuing as before, $\frac{1}{13}$ gr. of pilocarpine was injected into the arm. Soon the skin became moist, and in from 10 to 15 minutes patient was salivating, and was bathed in a profuse perspiration, great beads of sweat standing on the face, and, in the course of 15 minutes after the injection, and an hour and 25 minutes from their first appearance, the fits ceased and never recurred. The patient did not recover consciousness, however, until another hour and 25 minutes had elapsed.

During the fits both pupils were very much dilated, the left being slightly the larger, and the conjunctival reflex was absent. After the fits, and during the sleep and partial coma which followed, both pupils were contracted, and the conjunctival reflex slowly reappeared, first in the right eye and then in the left. The pulse varied much, both in tension and in rate—in tension, since at one time it was firm, hard, and incompressible, and at another extremely soft and only counted with difficulty, and in rate from 144 to 160. The respirations were also very irregular, both in force and rhythm, and they continued to be so for seven and a-half hours after the cessation of the fits.

Up till the onset of the fits the temperature was absolutely normal: two and a-half hours after the onset it was 101°, an hour later it was 101·2°, and three hours later still it was 99·8°, after which it remained perfectly normal. On regaining consciousness, the patient, whilst able to distinguish voices, was at first quite unable to see. During the night he passed seven ounces of dark, smoky urine.

The condition of the urine rapidly improved, and on the second day after the fits, it was found to be very pale and clear, nearly neutral in reaction, of sp. gr. 1005; there was the slightest sediment, which was found to consist of a few red blood corpuscles and leucocytes, some of the latter being highly granular; there was only a trace of albumen; the urea amounted to ·45 per cent, which was at the rate of 72 grs. in the twenty-four hours. On the evening of the same day the sp. gr. had risen to 1015, and the urea to 1·75 per cent, whilst the sediment contained, besides blood and leucocytes, several well marked epithelial casts. For the four following days the quantity varied from 39 to 46 ozs. (the average being 43 ozs.), the sp. gr. from 1010 to 1016, the percentage of urea from 1·1 to 1·8, and the calculated amount from 253 to 336 grs. (the average being 292 grs.); the albumen was in small quantity, blood always being present with leucocytes and occasional hyaline and granular casts. During the next three weeks blood was always present in minute quantity; there was a faint trace of albumen; the quantity of urine varied from 42 to 64 ozs., the average of sixteen consecutive observations being 57 ozs.; the sp. gr. varied from 1008 to 1011, for the most part being 1010; leucocytes were persistently present, but only very occasional casts were found; the urea varied from ·9 per cent to 1·25 per cent, and in calculated amount from 267 to 342 grs., the average daily excretion being 301 grs.

For the first time, amongst frequent observations, albumen was noted as absent on 15th April; but at many subsequent examinations a faint cloud on boiling, rendered more distinct by acetic acid, but absolutely without reaction with nitric acid, attracted attention. After a considerable interval, during which the boy had been away for a change and had returned to school and was enjoying good health, the urine was again examined on 17th October, and found to be of sp. gr. 1023, acid, pale greenish-yellow in colour, with slight mucoid sediment containing only a few epithelial cells of uncertain origin. There was 1·2 per cent of urea, and on examination, an unquestionable, though very faint, trace of ordinary albumen was found, giving the usual reactions.

This case, no doubt, was one of glomerulo-nephritis, where the tissue changes were largely confined to proliferation of the nuclei of the connective tissue cells, in the glomeruli of the kidney. The pathological changes in the epithelium were not apparently sufficient to permit of the exosmosis, in any great quantity, of the albuminous and fibrine forming elements of the blood, and yet the pressure in the Malpighian tufts was such as to keep up almost continuous hæmorrhage for weeks, and, at one time, so to interfere with the circulation in the kidney as to gravely endanger life. The convulsions were preceded by disturbance in the sympathetic system, as evidenced by the peculiar flushings of the face, by interference with the intellectual centres in the brain, as shown by the lethargy, and by irritation of the root of the pneumogastric, such as is found in the early stages of meningitis, as indicated by the somewhat slow and intermittent pulse. They were attended by absolute unconsciousness, dilated pupils, clonic spasms, and a rapid pulse such as is found in the latest stage of meningitis, when there is paresis of the pneumogastric. And they were followed by the comatose sleep which succeeds an epileptic convulsion, and by such an irregularity, both of pulse and respiration, as to indicate grave disturbance in the region of the medulla.

These symptoms would almost seem to favour Traube's view that the signs of uræmia are due to pressure effects, the result of some sudden development of œdema of the brain and its membranes. And it is difficult to understand, on any other hypothesis, the remarkable effects of venesection in many of those cases. If the symptoms were due to the *direct* action of the poison, why should the abstraction of a small quantity of it in the blood produce such a wonderful change in the condition and prospects of the patient? Whereas, if it be due to œdema, it is quite within our experience that a re-absorption of the effused fluids would at once follow the abstraction of blood from the arm, or of the watery constituents of the blood through the sweat glands. As mere pressure effects fail to solve the problem of the presence of albumen in the urine, so a purely mechanical theory fails to explain the various general œdemas with which we are familiar; and we are almost compelled to believe that pathological changes in the cellular wall of the capillaries and minute blood-vessels have a great deal to do with their production.

What, however, in uræmia is the cause of this œdema of the brain is evidently as yet an unsolved problem. That it is not urea seems pretty certain, else uræmia would be a much commoner incident than it is. I recently had the curiosity to examine for urea a sample of serum obtained by Southey's canula from the leg of a patient in the last stage of Bright's disease, where the œdema was very great, and was astonished to find that it hardly responded at all to the hypobromite of soda test; yet here there was an almost complete suppression of urine, the only relief obtained being by the bowels.* It seems reasonable, therefore, to suppose that the poison of the uræmic condition consists not in urea or in carbonate of ammonia, but in some intensely active alkaloid derived from the decomposition of the urea or other nitrogenous waste product, and that it acts on the vascular system of the encephalon so as to permit of the production of œdematous exudations, which in their turn give rise to the symptoms of uræmia.

It is interesting to note the very small amount of urea in the first sample of urine examined—*i. e.*, on the second day after the cessation of the fits, and its rapid increase in quantity on the evening of the same day. The excretion of urea at once reached an amount (first an average of 292 grains, and then of 301 grains in the 24 hours), which is surely very high in a boy of eight, who was confined to the recumbent posture and to milk diet. Only on one other occasion did the amount of urea fall low—viz., to ·55 per cent; and then again it was associated with livid flushings of face and slight lethargy.

The albumen, always slight in quantity, was unfortunately very persistent, and has only been absent at one observation. For long, however, it was in such minute quantity that it was only detected by the most careful boiling of the upper layer of the urine in a test-tube, and sometimes even then only on the addition of acetic acid; and on these occasions it never responded to the nitric acid test, even when most carefully applied by means of a pipette. When examined on October 17th, the reaction with nitric acid was again quite distinct, though very slight.

Therapeutically considered, it is very interesting to find that when venesection to a very fair amount had failed, the pilocarpine first showed its marked physiological effects, and then stopped the fits, and that after such a lapse of time as quite to put it beyond all reasonable doubt that the venesection had very little to do with the successful result. Indirectly, perhaps, it aided it, as did also the application of the hot pack, which was kept up very industriously; yet both of these methods of treatment were in operation for upwards of one hour and a half without in the least modifying the fits.

The case is one of those which cheers the physician on his way, and bids him hope and act, however hopeless and desperate the conditions with which he has to contend may be.

* The serous fluid was slightly opalescent, faintly acid in reaction, with a specific gravity of 1006, and it contained no albumen.

THE RESULT OF TWO YEARS' EXPERIENCE IN CONNECTION WITH A PROVIDENT DISPENSARY.

By ALEXANDER STEWART, M.D.,
One of the Medical Officers to the Pendleton Provident Dispensary.

Towards the end of 1880, I became connected with the Manchester and Salford Provident Dispensaries Association, after having been invited to do so, as one of the medical officers to the Pendleton Provident Dispensary. For several reasons, chief among which was the alleged inadequate remuneration paid to the medical officers, the Provident Dispensaries were not regarded at that time with any favour by a majority of the medical profession in the district. Being convinced, however, that the scheme was based on a sound principle, I consented to undertake the duties of medical officer, and do what I could to give the scheme a fair chance of succeeding, believing that a practical knowledge of its working would alone show where its defects lay, and at the same time indicate in what directions improvements were capable of being effected. After working in connection with the Dispensary for some time, it occurred to me that a useful and interesting thing to do would be to keep a daily record of all consultations and visits to members of the Dispensary, which record of actual work done by a medical officer would give reliable data with which to compare the remuneration received by him. Though statements have been freely made regarding the inadequacy of this remuneration, I am not aware that hitherto any reliable data have been given from which strictly accurate deductions could be drawn. This record also brings out other points of interest in connection with the working of the system when reduced to practice. But before proceeding further, it will be well to state briefly what the main provisions of the Provident Dispensary scheme are, for the enlightenment of those who are not acquainted with them.

The Rules state that the aim of the Association is to provide medical relief for the working classes of Manchester and Salford upon the provident dispensary principle, by establishing dispensaries in suitable districts of the city and borough, and to endeavour to obtain the co-operation of the Medical Charities in the work. The business of the Association is managed by a Council composed of representatives from the District Provident Society, from the Medical Charities connected with the Association, and from each Provident Dispensary; and of others elected by these. The aid of the District Provident Society has been indispensable to the Association, as it has been through that society that the sinews of war were provided for the establishment of the dispensaries. Those entitled to the benefits of the dispensaries shall be artizans and others in like pecuniary circumstances, whose application for membership shall be approved by the local committees (but as healthy artizans are usually members of clubs who have surgeons of their own, it is mainly for the wives and children that the dispensaries are useful). Every member above 14 years of age pays one penny per week subscription. Children under 14 years of age can only be admitted with their parents or guardians (usually their mother), and the first two next under 14 pay each a penny per week; but this subscription of twopence weekly also entitles any number of younger children in the same family to the benefits of membership. The father of a family is not compelled to join with his family, if he is already in a club. There is an entrance fee of sixpence for an individual, and of one shilling for a family of two or more. If the applicant for membership be sick, he shall pay an entrance fee of five shillings, and there is a similar proviso if a member require the doctor's services within a fortnight of admission; but on the expiry of two weeks members may come under treatment without further payment. All subscriptions are due in advance, being paid weekly, fortnightly, or monthly, as the case may be, and members in arrears are not entitled to the benefits of the institution. One penny is charged for every prescription made up. A member can choose his own doctor from the dispensary staff. Members can have the services of their doctor in their confinements for a fee of fifteen shillings, to be paid one month before the expected time of confinement, or of the midwife for seven shillings and sixpence, and the doctor shall assist the midwife, when necessary, for a fee of ten shillings and sixpence. Each dispensary is also supposed to have Honorary Members who should contribute yearly at least one guinea to its funds, but receive no benefit in return. Each dispensary is managed by a Committee composed partly of honorary members and partly of ordinary members of the dispensary, with the medical officers. The funds of a dispensary are disposed of as follows:—The medical officers receive half the members' subscriptions and half the entrance fees, also the midwifery fees for the cases they attend; all the remaining income goes towards the expenses of management, and if there be a deficit, the District Provident Society, through the

Council, is responsible for its payment; but if instead of a deficit there be a surplus, it is provided that at least two-thirds thereof shall be divided amongst the medical officers in proportion to the amount already received by each.

When this Scheme is reduced to practice it is found to work unfairly in some of its details. It may be granted that, taking a large community, every member of which pays a penny per week for medical services, this would amount in the aggregate to what would pay the doctor moderately well proportionately to his work; but for this to hold good two things are first necessary, viz.:—(1) That the community should be an average healthy one; and (2) that every member of it should subscribe. In the scheme before us, however, 13 per cent of the people are admitted free, and these too the young children who ordinarily require medical aid, in proportion to their number, much oftener than it is required by the class of persons one would expect to find represented by the paying members. There is internal evidence in the rules which shows that the framers of the scheme held that the payments by members should bear some relation to the work likely to be required of the doctors, or, in other words, that a member should pay in proportion to the benefits he is likely to receive. Therefore, if a person is sick he must pay five shillings, or more than a year's subscription, to enter the dispensary, because, presumably, he will give trouble and receive benefit immediately. But, if a person pays sixpence to enter and waits a fortnight (as is very frequently done) he has saved four and sixpence, or more than a year's subscription, and can have what medical services are necessary. As a rule, it is acute cases that pay the five shillings entrance fee and are well soon after; but the chronic cases that have often been going on for years pay the sixpence entrance fee, wait fourteen days, then come under treatment and remain there. It would, therefore, be desirable to make some alteration in this rule, either to abolish sick entrance fees altogether, and thereby, as things work at present, remove an injustice, or, leaving sick entrance fees as now, make it so that a person must be a member for more than fourteen days before being entitled to the benefits of the institution. Again, the principle practically admitted in establishing sick entrance fees is violated in receiving any possible number of children under 14 years of age in one family for the same subscription as two. There are families with six, seven, and more children under 14, and all these are entitled to the privileges of membership, so far as the doctors are concerned, just the same as if there were but two of them. This excellent arrangement—excellent from the members' point of view—does credit to the kindliness of heart of those who made it, but its financial aspect won't bear the light. No arguments in favour of promoting habits of providence can get over this, and to cover it the active charity of the doctor is drawn upon. On the other hand, it may be questioned whether a man with a numerous young family, the man himself being the only breadwinner, could afford, or would be willing to give, perhaps sixpence or more weekly out of his earnings in order to provide medical aid whenever required. A man, anxious to pay his way and strongly impressed with a sense of the necessity of prompt medical aid, as a requisite for a family's wellbeing and comfort, might do it; but many would prefer to spend the money on a drink of beer at the week end, thus receiving an immediate return for it, while, if invested to insure against doctors' bills, it would only yield a prospective return; while in health we all know how careless one is apt to be in making provision for sickness. It then comes to be a question as regards a large proportion of the working classes, whether all their children are to be charged for and incur the risk of not reaching them by the provident dispensary system, on account of the subscriptions being higher than they are able or willing to pay, or admit them as at present by paying for two only, thereby securing a larger accession of members to the dispensaries, at the expense of more work for the doctors, with less proportional remuneration. The latter course is the more charitable and humane, and it is easy to exercise charity in theory merely; but when it comes to the sublime drudgery of visiting and attending the sick from day to day, receiving but little tangible reward, it puts one's charity to a practical test. It may be asked whether in these cases the exercise of this kind of charity is a virtue at all. Is there any virtue in giving an article to a man for less than its real or market value when he is able to pay the full value for it? Certainly not. The difficulty is in judging accurately of the man's ability to pay the full value. That being so, it is safer and better to err on the side of leniency. The children have first to be clothed and fed, and when that is done there may be but little left to provide for luxuries, even for such a necessary luxury as medical aid, and it would be a pity that the inability to adequately pay for such a luxury should constitute a barrier to its bestowal. So that in every case it would not be wise to insist upon all young children being paid for. A middle course would seem the more feasible, and as a guide to this middle course the house rent might be taken as an indication. Indeed, the house rent would be in the first instance a better guide to follow as to who should be admitted to, and excluded from, the benefits of the dispensaries, leaving the details of particular cases to be decided by the committee. At present the committee have to deal with the matter of admissions; but this duty may be, and is, shirked, with the result that every applicant is admitted unless the medical officers take upon themselves the odious duty of objecting when they have found out the improper character of any case.

Another detail in which the scheme works unfairly, and which gives it a tendency to degenerate into a charitable agency, is, that every encouragement is afforded to the admission of an unduly large proportion of old and infirm members. Far too large a proportion of such join the dispensary—perhaps it is the sickly member of a family, or the old mother-in-law, or any one that can't get on long without the aid of a doctor, that is entered, and often the healthy members of the household don't think of entering unless there are premonitory signs that some of them too will require medical aid. Consequently, the members of a provident dispensary consist—(1) largely of infirm persons, (2) of far more than a fair share of the aged, and (3) of females, (4) many young children, with (5) but a minority of healthy men, because these are for the most part in clubs. And there is a uniform fee of one penny per week for every paying member, no matter how old or decrepit, and no matter how healthy. It would seem but fair and just to all concerned to make some difference in the subscriptions, as there is very great difference in the amount of work the different classes give to the doctors. The precaution taken to charge one penny for every prescription made up saves the dispensary from any loss, for it is found that this charge in the aggregate rather more than covers the original cost of the drugs, so that, at the end of the year when the accounts are made up, it matters not to the financial condition of the dispensary how many chronic patients there are. The medical officers bear the brunt of the extra work, of which no official notice is taken, and things are considered to be perfectly satisfactory if the year's income can be made to cover the year's expenditure.

At first thought, one might be apt to consider that members' subscriptions (the medical officers receive only half) would be sufficient to pay the doctors a fair remuneration when it is remembered that sick clubs are attended and supplied with medicine by their surgeons at the rate of three shillings per member per annum. There is, however, little analogy between the two cases, as these clubs are entirely composed of working men who have to pass a medical examination and be certified sound, and a healthy man does not require much physic from year's end to year's end.

It would be easy to give details of individual cases that have made much work for the medical officer—many such cases are constantly under treatment; and it would also be possible to mention cases that have never come under treatment during the two years; but it is considered better to give the results as a whole without particularising single cases. Therefore, in order to show at a glance the work done with the remuneration received, and their relation to one another, the accompanying table has been drawn up. It covers a period of two years, and gives the results of every succeeding half year, because the books are made up and the doctors paid at the end of the half year. The fees received for confinements, and odd shillings (which the rules allow) for extra visits and certificates, are added to the half-yearly dividends because it seemed fairest to do so, it being sometimes difficult to say when the visits for the confinements alone began or ended, as it happened on more than one occasion that there were complications necessitating more than the usual number of visits in such cases—indeed, it is the expectancy of complications that sometimes induces the engagement of the doctor; when no complication is expected the great majority are satisfied with midwives. Therefore, in making up the list of visits to dispensary patients, all visits are recorded, as are also all payments. The consultations and visits to non-paying members are distinguished from those to paying members to bring out the ratio between the two classes as regards sickness. One would naturally suppose that non-paying members, being all young children, almost all of them being under ten years, and the majority of them under five years of age, would have more sickness in proportion to their number than the paying members, from the well known fact that persons of such tender years are more liable to sickness than those above that age, and also have a higher death-rate. But such a supposition —that a greater amount of sickness would exist among non-paying than among paying members—however natural, is found to be erroneous. During the two years covered by the table—that is, from 1st July, 1881, to 30th June, 1883, 2,308 members joined the Dispensary, and of that number 301, or 13 per cent, were non-paying. During the same period 12,096 consultations and visits were recorded, and of these 1,115, or

Table showing the Number of Visits and Consultations for the Two Years Ending 30th June, 1883, with the Payments Received during the same Period.

	Number of Visits.			Number of Consultations.			Total Number of Visits and Consultations.	Remuneration.			Average Payment for each Visit and Consultation.
	Paying Members.	Non-paying Members.	Total.	Paying Members.	Non-paying Members.	Total.		Share of Members' Subscriptions.	Confinement Fees and Odd Shillings.	Total.	
Half-year ending 31st Dec., 1881,	568	80	648	615	47	662	1,310	£21 4 6	£3 6 0	£24 10 6	4·493 Pence.
Half-year ending 30th June, 1882,	757	190	947	1,547	124	1,671	2,618	44 15 0	3 16 0	48 11 0	4·450 ,,
Half-year ending 31st Dec., 1882,	1,260	101	1,361	1,872	153	2,025	3,386	63 1 4	5 8 0	68 9 4	4·557 ,,
Half-year ending 30th June, 1883,	1,562	230	1,792	2,800	190	2,990	4,782	92 17 7	3 6 0	96 3 7	4·827 ,,
The two years, .	4,147	601	4,748	6,834	514	7,348	12,096	£221 18 5	£15 16 0	£237 14 5	4·716 Pence.

It is seen from this Table that, of the 12,096 consultations and visits, 1,115 (601 and 514), or 9·2 per cent, were to non-paying Members.

A "consultation" means that a patient comes to the doctor to be prescribed for, and the number of persons thus seen is correctly represented by the number of consultations. A "visit" means that the doctor goes to a house to see the patient or patients, for there may be two or three sick persons at the same time in one house, and these are only credited with one visit, so that the number of patients seen at their own homes is not correctly represented by the number of visits recorded.

9·2 per cent, were to non-paying members. Thus, in numbers the non-paying members are 13 per cent of the whole number of members, but their sickness is only 9·2 per cent of the whole sickness. What does this demonstrate? Does it not conclusively prove that the ordinary members of the Dispensary are not average healthy members of society? People are not inclined to join the dispensary unless they have some motive, and the chief motive is a chronic ailment. In illustration of this point, take the following example:—A sickly, most likely a phthisical, young woman, who alone of the family has entered the dispensary, is brought for treatment by her mother, who, when asked, "Why don't you all join?" replied "Oh! we are never ill scarcely!" Being evidently impressed with the idea that only sick persons, or such as more or less constantly required medical aid, should be connected with a dispensary. This idea is generally current among the people, and is partly due to their hitherto exclusive acquaintance with the purely charitable dispensaries, and partly also to the dislike of spending money for a prospective benefit. In the case of the free dispensaries it is only the sick that are immediately concerned, the others know that if they too should be equally unfortunate they can easily get a "recommend" from the charitably disposed, and that, therefore, while in health, they need not trouble about these matters; they consider it no degradation to ask and to receive for nothing what is received by so many as a matter of course. No doubt in time the working of the provident dispensaries will tend to instil a different and healthier sentiment into the minds of the people, and let them see that it is to their own advantage to assure themselves of capable medical aid in times of sickness. But it is the case now, and it is to be feared always will be, that an abnormally large proportion of the members are decrepit. At the same time, there is a tendency towards improvement in the general health of the members as a whole. It is seen from the table that, while in the first half year the average sum received per visit or consultation was 4·493 pence, in the fourth half year it was 4·827 pence, and the average of the two years 4·716 pence. Let it be noted that this is in connection with a dispensary that has not hitherto paid its expenses, and that therefore the doctors received a minimum payment; if there were a surplus the doctors would receive two-thirds of it. The improvement in the general health of the members is likely to be more marked as time goes on, and several agencies are working towards that end. Many of the working classes know how costly a luxury medical attendance is, and, if honest, they are anxious to pay for what they get; and to avoid incurring a debt, which would be a burden or an impossibility to cancel, they join the dispensary. Not unfrequently also the whole of a household joins, when it is seen that the sickly member of that household, who has been already receiving the benefits of the institution, is really well treated: there have been many such cases. Also persons who have a chronic disease are often so far benefited under treatment as to be able to do for a time without medicine and so give but little trouble.

Another cause, beyond what has been already stated, of the large amount of work in proportion to the number of the members is, that they come for treatment when not seriously ill. One meets with more cases of slight ailments than in private practice. And this is undoubtedly one of the great recommendations of provident dispensaries. The nominal charge of one penny for medicine does not deter the coming forward of the patients at the first symptoms of disease, and in this way a great amount of suffering and illness may be, and is, avoided.

An important matter for the consideration of the promoters and supporters of the Provident Dispensary Scheme is, whether at the present rate of remuneration they can always secure good men as their Medical Officers. Without the latter there can be no real or permanent success. Some means should be adopted by which their services can be retained for a longer period than they have hitherto been retained. Medical men have been encouraged to regard these dispensaries, even by their promoters, simply as stepping stones which are to be left behind as soon as they have served to place the doctors in a position where they are able to do without them, having attained to a sufficiently large private practice. This is a very unwise policy, which, while practised, will result in the dispensaries living a chequered and uncertain life. Those Medical Officers who have been long associated with the dispensaries, and have, as it were, earned their spurs, should be appointed Consulting Medical Officers, whose duty only would be to sit for consultations, the visiting to be done by men more recently appointed, or even specially elected for that purpose. Should this or some similar plan be adopted the result would be to give greater stability and permanency to the success of the movement. It will not readily be asserted that the mere monetary rewards, above indicated, will tempt many to aspire to the honour of becoming medical officers to the provident dispensaries. The offer of 4·716 pence for every visit and consultation is more likely to repel than to attract. Nevertheless, to a man in love with his work there are inducements besides the mere monetary one, if that be one, to connect himself with a provident dispensary. A matter of the first importance to a young practitioner, who has a name and reputation to make is, that he should have a field wherein to work, and have opportunities of showing his aptitude for work. A beginner in private practice, even though he should nominally succeed another practitioner, has not all the opportunities he could desire of showing his skill; and as patients are more or less sensitive on the point of expense they wont consult him oftener than they think necessary and give up coming when feeling a little better, but perhaps only half cured—not to the credit of the doctor. But, as members of a provident dispensary, the only point that weighs with the patients is to get well, and the question of fees does not interfere, they therefore remain under treatment till quite cured, or as long as the doctor thinks it necessary. A great inducement, therefore, for a young practitioner to become connected with a provident dispensary is the field of work that is likely to be opened up for him, the opportunities thereby afforded of observing and studying disease, and the ever accumulating experience gained in its treatment; and all this without the dispensing, book-keeping, and similar drawbacks inseparable from private practice. The experience gained by a medical officer to a provident dispensary is more valuable than that gained at a free dispensary, because at the latter a patient, when cured or otherwise lost sight of, is, perhaps, never seen again; not so with a member of a provident dispensary. After being cured once, the patient comes again for the same or some other complaint, and remains in this way more or less constantly under observation, and the ultimate, as well as the immediate, results of treatment made known. Another point also, it may be permitted to mention, as a set off against meagre monetary reward, and it is this:—The satisfaction one feels in doing what he believes to be a good work, and endeavouring to be of help in the curing of disease, in the relief of suffering, in the prolongation of human life.

One result of the existence of Provident Dispensaries in Manchester has been a reduction of the abuse of the Medical Charities. The managers of the latter have permitted enquiries to be made into the circumstances of their applicants for treatment, with the result that the number of out-patients in 1881 was 41 per cent less than in 1872. This 41 per cent now go somewhere else for medical aid; some, no doubt, go to the provident dispensaries, but the majority to private practitioners. In this way the provident dispensary movement, instead of being injurious to private practitioners, has conferred a distinct benefit upon them; and even if there were no gain of this kind—viz., the reduction of the abuse of medical charities—the comparatively small number of the whole community who are members of the dispensaries would surely have no great effect in reducing the income of practitioners generally. There are ten provident dispensaries in Manchester and Salford, scattered over the whole area, and the gross number of members among the ten, on 31st December, 1882, was only 14,115, and this in a population of over 560,000. In Pendleton district alone, with a population of over 48,000, the members of the provident dispensary are something over 2,000, or about 1 in 24, and of that number not a few come from outside the township boundaries. So that the injury done to practitioners who have no connection with the dispensaries, in the taking away of their patients, must be very small. It has been stated that some of the medical officers did not wish the dispensaries to succeed. Be that as it may, one can easily understand how hostile feelings might be engendered in the minds of medical men against these dispensaries—when, for example, a medical officer has been attending a patient, and receiving at least half-a-crown for every visit, and the next time he attends the same patient it is as a member of the provident dispensary. Some pardonable temporary irritability may be caused in this way; but on further looking into the matter, it is seen that the fact of the patient joining the dispensary may not be altogether a disadvantage, even from a pecuniary point of view—that is, if the person enjoys average good health; for now the patient is, as it were, secured to the medical officer, and is a continuous source of income, if at a small rate and at the expense of much work, while formerly there was no continuous payment, but only when the need of relief became urgent. Therefore, taking a large number of cases, not individual ones, it is probable they pay the doctor as much being members of a provident dispensary as if they were attended privately. In the latter case they only come when obliged; in the former they don't put off coming so long, and therefore give more work. It should be the aim of the medical officers to prevent, as far as they can, the abuse of the dispensaries, for they are liable to imposition as much as the medical charities. If the local committee is at all remiss in checking admissions, improper cases are occasionally allowed to enter; for the medical officers to object to these is an unpleasant and odious task, but should not be shirked nevertheless. In Pendleton several such cases have occurred, and the members who had no right to be admitted, being able to pay private fees, have been eliminated. Whenever I attend working people as private patients, especially those with families, I recommend them to join the Dispensary, because I find it more satisfactory to attend them as such than attending them privately and subsequently giving the bill to a collector to get the money by weekly instalments or otherwise as he can. Such a method I dislike very much, finding it unsatisfactory and troublesome, and believing it also to be derogatory to the self-respect and dignity of the medical profession.

The extension of provident dispensaries should be encouraged by the promoters of sanitary science, as a force capable of materially aiding them in spreading a knowledge of the laws of health, and of generally improving the hygienic condition of the people. If these dispensaries were more largely taken advantage of by the classes for which they are intended, there would be fewer cases of death the causes of which have not been certified by a medical practitioner. In Salford,* during the year 1882, of 4,265 deaths 193, or 4·5 per cent, were buried by order of the coroner, and 262 persons, or 6·2 per cent, died without medical attendance. Had these been connected with a provident dispensary, if the deaths could not be prevented, they would at least be properly certified and a scandal removed. In provident dispensary practice, the object of the physician and patient is the same—viz., that the patient should get well as quickly as possible, and remain well; whereas in private practice there are temptations for the patient to keep down expense, and therefore to abstain from seeing the doctor as often as might be desirable for the patient's own sake; and on the other hand, there are temptations for the doctor to keep the patient under his care longer than may be absolutely necessary, for the sake of the fees. Of course no man of principle and right feeling would do such a thing, yet that does not alter the fact that a man of an opposite character, with the selfish side of his nature largely developed, could do so if he chose. It cannot, therefore, be denied that it must be a good thing when the only object the physician *can* have is to get his patient well and keep him well, and when the patient has no deterring influence to keep him from consulting his physician as often as he feels the need of it. Therefore, the *morale* of the provident dispensary system is good in its effects upon

* See Dr. Tatham's Annual Report of the Health of Salford, 1882.

the medical man, and good in its effects upon the members besides encouraging in them habits of thrift and self-dependence. That the system is perfect few would pretend to say; that it contains much that is good, deserving of being fostered and extended, no one will deny. The system has, as it deserves to have, strong and influential supporters who will not readily succumb to any opposition. They have nursed it in its infancy; it is scarcely a lusty child yet, and barely able to walk alone, but indications are not wanting of future health and vigour. It cannot be gainsaid that this result can only be achieved by the faithfulness of the medical officers to their trust. Their duty and their wisest policy, as of the medical profession generally, is to co-operate in the promotion of the provident dispensary system, to endeavour to eliminate what may call for elimination, to amend what may require amendment, to nurture what is found to be good, so as in time to attain to a scheme more perfect as a whole and calculated to confer vast and far-reaching benefits upon their fellow-men.

In conclusion, it is but right to say that in professional dealings with the members of the provident dispensary, I have invariably met with every civility and respect, and a general disinclination to give more trouble than was necessary. Their expectations have not been apparently too large to be impossible of realisation; the percentage of dissatisfied cases, so far as I know, has been but very small. The Committee have always shown a readiness and willingness to meet the doctors' wishes that cannot be too highly commended, and while anxious to promote the welfare of the dispensary, by attending to the interests of its members, are ever watchful of the respect due to their medical officers.

RESECTION IN FRACTURE.

By JAMES PATON, M.D., Greenock.

Case I.—J. L., aged 26, engineer, was admitted into the Greenock Infirmary on 18th April, 1880, twelve days previous to my taking charge of the wards. The injury was inflicted in attempting to commit suicide by jumping over a high railway bridge on to the rails. On admission, it was found he had sustained a severe compound fracture of both bones of the right leg, the upper fragment of the tibia protruding through the wound. The fracture was reduced, dressed aseptically, and put on a M'Intyre's splint. On the 25th the limb was redressed and found in fairly good position. On 3rd May the dressings were removed, when it was found the upper fragment was again protruding, and could only be partially reduced. A pad was placed over the upper fragment with the usual gauze dressings and bandaged, and again placed on the M'Intyre's splint. For two or three days before this he was very restless and excited, trying to get out of bed, and on one occasion nearly managed it. On the 8th the dressings were again removed, when it was found the upper fragment was still protruding. He was anæsthetised, and as reduction could not be satisfactorily effected, the protruding portion was cleared and sawn off; the limb was then got into fairly good position. On the 11th it was again dressed and found in good position. There was no constitutional disturbance, his temperature and pulse remaining normal. After this he was very carefully watched, as he sometimes got so excited as to make it absolutely necessary to strap him to the bed. All knives and instruments by which he could injure himself had to be kept from him. The limb was not again disturbed till the 23rd, when it was found in good position; the granulations being a little flabby were powdered with iodoform. After this, it was dressed every sixth day, the discharge remaining sweet all through until the 26th July, when, the wound being superficial, and good union having taken place, the aseptic dressings were left off and the limb put up in a gum and chalk bandage, with a window cut in it, for the purpose of dressing the wound with simple boracic lint. On the 3rd August he was discharged and sent to an asylum. He walked down the Infirmary steps without any assistance.

Case II.—W. M., aged 39 years, received a compound comminuted fracture of left leg, the result of a railway collision. He was admitted into the hospital on the evening of the 8th September, 1880, being carried from the station on a barrow. He was pale and faint from the loss of blood, but did not labour much under the influence of shock. On examination it was found he had received a severe compound comminuted fracture of both bones of the left leg about the middle, the wound communicating with the fracture being very small. At the lower third there was also a simple fracture of the tibia. On the external aspect of the foot there was a large flesh wound 3 in. by 2 in., but which did not communicate with the joint or fracture. The fractures were reduced and the limb got in fair position, the comminution being so great as to render exact coaptation impossible. The upper fragment had a tendency to protrude, so that a pad of gauze was put along the anterior surface, the whole dressed with the usual aseptic dressings under the spray, and the limb placed on a M'Intyre's splint. He passed a comfortable night, and on the following morning expressed himself as feeling well. There being a slight amount of discharge through the dressings they were removed, when it was found that a pretty large comminuted portion was lying loosely under the skin, causing slight tension. The limb was redressed as before. It was not again touched till the 15th, when there was a little discharge through the dressings, and he complained of pain and heat at the seat of compound fracture. The dressings were accordingly removed, and the skin over the loose fragment above mentioned was red and tense. To prevent further destruction of tissue, I incised over this fragment and removed it, preserving the periosteum. It was from the anterior surface of tibia, $1\frac{1}{4}$ in. long by $\frac{3}{4}$ in. thick, communicating with the medullary canal. A large blood clot was adhering to the internal surface in process of organisation. The limb was again dressed as before. The patient expressed himself as feeling perfectly comfortable and free from pain. For the next three weeks the limb was left undisturbed. On the 5th October the dressings were removed and the compound fracture was found reduced to a simple one with a good amount of bony union, the wound entirely healed. He was discharged on the 11th November, previous to which period he had been getting up and walking with the aid of crutches. Osseous union was then complete. The flesh wound on the external aspect of the foot gave no trouble, healing under the infrequent and aseptic dressings.

Case III.—H. M., aged 39, labourer, was admitted on 31st January, 1883, to the Infirmary, suffering from a fracture of both bones of the right leg, caused by a plank of wood being driven suddenly and forcibly against the limb. The fracture was comminuted, a piece of the tibia being evidently knocked posteriorly among the fibres of soleus or gastrocnemius, there being a complete gap in the continuity of the tibia, with no bony substance to be felt in it. After extending, the limb was put up in side splints. On the 4th February, there being so extensive mobility in the limb, and this gap with nothing to be felt in it, I considered the chances of good bony union so small, that it was decided to resect. The patient was put under chloroform, an incision was made sufficient to expose the ends of the upper and lower fragments. It was then found that several comminuted portions were lying loose in the tissues; one of them, $1\frac{3}{4}$ in. in length, nearly the whole thickness of the tibia, was found imbedded in the muscular fibres behind. The comminuted portions having been removed, the ends of the bone were sawn off, drilled, and drawn together with strong silver wire, a drainage tube inserted, the incision stitched with horse hair, dressings applied, and the limb put on a M'Intyre's splint, all being done with the strictest aseptic precautions. It would serve no purpose to give the details of the progress of the case, as the main facts with which we have to deal may be summed up as follows:—There was no constitutional disturbance of any kind throughout, his temperature and pulse remaining normal. The discharge was so slight that the dressings were not removed, on an average, more than once in 12 days or a fortnight. The wire was removed on 13th March. On 20th April, osseous union very firm. On 3rd May, able to lift up the leg without pain. On the 26th May, dressed with simple boracic lint, able to move the leg freely. Discharged on 16th June, able to walk comfortably.

Remarks.—If there be any class of cases which has been brought forward over and over again to prove the value of aseptic surgery, it has been that of compound fractures. The above cases show to what extent aseptic surgical interference with bony structures may be made, and productive of the happiest results; and it seems to me that resection has not yet attained that general consideration in the treatment of severe compound fractures, and even of some rare forms of simple fractures like Case No. 3, which ought to be given to it. Many limbs might be saved in this way, which otherwise would be amputated. Of course there is bound to be shortening of the limb, but not more than the use of a high heeled boot will make good. There is one thing which I have found of enormous advantage in the treatment of severe compound fractures, and that is the moulding of an iron bar to the contour of the limb, of sufficient strength not to yield when the wound is being dressed.

I have given those cases, not for the purpose of lauding the aseptic treatment of fractures (as it is now too late in the day to do so), but more for the purpose of showing what beneficial results may be obtained from resections conducted on aseptic principles.

CORRESPONDENCE.

Collective Investigation Committee. British Medical Association—Glasgow Branch.

2 India Street,
Glasgow, *27th Nov., 1883.*

(To the Editors of the Glasgow Medical Journal.)

Gentlemen,—As is no doubt known to most of your readers, a Sub-committee of the Collective Investigation Committee was formed last year in connection with the Glasgow and West of Scotland Branch of the British Medical Association. Dr. Napier was appointed Secretary of the local committee, but finding himself at present unable to overtake the duties of this office he has asked me in the meantime to perform them for him.

I find that some time ago Dr. Napier issued a number of cards on several subjects, only a small number of which have been returned, and as most of the gentlemen to whom those cards were sent are likely to see this *Journal*, I take this method of asking those who have not yet sent replies to do so at their earliest convenience.

I have on hand cards on the following subjects:—I. Acute pneumonia. II. Chorea. III. Acute rheumatism. IV. Diphtheria. V. Syphilis, acquired. V*a*. Syphilis, inherited. VI. Acute gout.

I shall be very glad to supply copies to any who may wish them. It is specially desired to have as many returns on Acute Pneumonia as possible, as a final report is intended to be issued on this subject at an early date.—I am, gentlemen, yours &c.,

Wm. G. Dun, M.D.

Obituary.

The Late Dr. Foulds, Mauchline.

A few weeks ago there was laid in the beautiful cemetery, recently opened in this parish, all that remained of a townsman whose memory will long be cherished in the hearts of the people amongst whom he lived and laboured. The death of Dr. Foulds, a medical practitioner here for no less a period than thirty-five years, calls for more than a casual notice. Dr. Foulds was a native of Stewarton, and in early life devoted himself to the study of medicine,—receiving his professional training in Glasgow, where he eventually graduated. In 1848 he commenced practice in Mauchline on his own account, and soon afterwards came to fill the position which had long been occupied by the much respected physician and surgeon, the late Dr. Nicolson. Not only did the doctor straightway approve himself to his patients as one who had chosen his profession from the very love of it, but he continued to keep his medical knowledge abreast of the age; and this, with the experience afforded by an extensive practice, made him one of the most reliable medical advisers. He long ago received the appointment of Medical Officer for the Parish and Factory Surgeon for the Public Works, and these he retained till his death. He was for many years, and up till his demise, a member of the British Medical Association. Altogether apart, however, from professional accomplishments, Dr. Foulds was possessed of a many-sided nature which endeared him to a large circle of friends beyond his patients. His genial and sympathetic manner, his wide acquaintance with the *Belles Lettres* of the English language, and his readiness to co-operate in movements having in view the good of the parish, all conspired to make him a favourite with every class of society.

Dr. Foulds was Past-master of the Burns' Mother Lodge of Masons, and for many years he was President of the Mauchline Horticultural Society. It was his oft expressed wish that his last illness would not be a protracted one, and it was gratified. After two weeks' indisposition, in which the symptoms were mainly confined to an affection of the optic nerves, Dr. Foulds passed quietly away at the age of 64, leaving a widow, three daughters, and three sons, all of whom are grown up. Possessed of a mind which in matters of religious faith could not be readily bound down by dogmatic formulas, the doctor yet evinced in his life the genuine spirit of Christianity, and has gone down to his grave crowned with the honour which always attaches to a good man after a useful life.—*Communicated.*

REVIEWS.

On the Pathology of Bronchitis, Catarrhal Pneumonia, Tubercle, and Allied Lesions of the Human Lung. By D. J. Hamilton, M.B., F.R.C.S.E., F.R.S.E., Professor of Pathological Anatomy (Sir Erasmus Wilson Chair), University of Aberdeen. London: Macmillan & Co. 1883.

The present volume is the publication, in a collected form, of a series of papers which appeared in the *Practitioner* for 1879-80, revised and brought up to date by the author. The book is illustrated by a large number of beautiful woodcuts, which greatly aid the reader in following the text. Part I deals with bronchitis, and discusses in great detail its acute and chronic forms, the varieties of the chronic form, and the various complications of the disease. Besides this, the purely pathological portion, there is also a very complete account of the normal structure of a bronchus. The subject of Part II is catarrhal pneumonia and tubercle in the human lung.

The descriptions throughout the work of normal and abnormal naked eye and microscopic appearances are accurate, clear, exhaustive, and forcible; and these remarks specially apply to the accounts of the normal structures, the histology of which Professor Hamilton has most carefully studied and as carefully described. No one can read these portions of the book without feeling that he owes a debt of gratitude to the author for the careful elucidation of the subject contained in its pages, and without also feeling that his understanding of much connected with the morbid anatomy of the lung has thereby been rendered clearer. While, however, we have been greatly pleased with the purely descriptive portions, and think that it would be difficult to find more accurate and minute histological accounts anywhere, yet in the more theoretical parts we could not help feeling that they were pervaded by a tendency to dogmatise, which, especially in the case of tubercle, where it is so difficult to be dogmatic, was disappointing. So strongly did this feeling strike us in reading the book, that we were reminded of the advice sometimes given by clergymen to young men fond of reading heterodox volumes—study the other side of the question before you make up your mind. As regards the general pathological questions at issue, the reader will get, we fear, but a one-sided account, and will leave the book almost entirely unacquainted with the labours and opinions of other workers in the same field.

From this general statement of our opinion of the book as a whole, it will be readily inferred that there are many points concerning which we cannot agree with Professor Hamilton, to a few of which we would now refer in greater detail.

Among the affections "which so frequently complicate a simple bronchial catarrh," according to Professor Hamilton, " . . . one of the most important is chronic interstitial pneumonia." He believes that the two frequently co-exist, and looks, when this is so, upon the bronchitis as the *raison d'être* of the pneumonia. Now we are by no means so certain —except, perhaps, in the case of children—that interstitial pneumonia is such a frequent complication of a bronchial catarrh as Professor Hamilton would have us suppose. Certainly the reasons which are advanced as explaining the part played by bronchitis in the causation of interstitial pneumonia are most ingeniously stated, and have almost the effect of making us wonder how any patient suffering from chronic bronchitis can escape the complication. These reasons are given at pages 83 and 84, one or two sentences from which we may now quote: "From this, the cause of the interstitial pneumonia in chronic bronchitis will be apparent. The accumulation of inflammatory effusion which takes place in the mucosa is pent up by the resistent basement membrane. It cannot be thrown off into the bronchus, and consequently, in the course of time, the cellular structures, partly by their amœboid movement, and partly by the force of the lymph-stream, pierce into the peribronchial and periarterial lymphatics, and not only infiltrate but also excite them to inflammatory action, resulting in the production of the changes seen in figure 21. The same thing happens in the lobular septa." It will be seen from this that practically the cause of the whole condition is the resistent basement membrane, which prevents the inflammatory products from getting into the lumen of the tube. But we are inclined to ask, Is it necessary that these products should pass through the basement membrane in order to be expectorated through the bronchial tubes? Is it not possible that if the products are present in greater excess than the lymphatics can easily dispose of, the fluid portion may pass through the walls of the alveoli, and the cellular elements also obtain access to the same spaces in virtue of their amœboid movements?

Under the heading of "The effects of increased blood pressure suddenly applied to the blood-vessels of the lung," we find some rather unusual views advanced as to the pathology of inflammation. At page 114 this occurs:—"All these phenomena, to which we give the name of 'inflammation,' I look upon as *purely mechanical*, and as resulting from one cause, namely, *suddenly increased blood pressure*." (The italics are the author's.) Now we think that no one who has entered into the spirit of Cohnheim's great researches, or who has studied the subject practically in the web or mesentery of the frog, will be inclined to agree with Professor Hamilton in this very dogmatic assertion—at least in its entirety. "From the study of the frog's web and the mesenteries of different animals, we know what the effect of an irritant suddenly applied to these tissues is. The arterioles spasmodically contract, and remain in a contracted state for a length of time proportional to the intensity and duration of the irritation." Personally, we have frequently watched the phenomena of inflammation in the web, and were never able to satisfy ourselves that a contraction, unless, perhaps, a very transient one, occurred at all, and Cohnheim has drawn attention to the fact that in the mesentery the reflex spasm is usually entirely awanting, and is often so elsewhere. According to the careful observations of Saviotti, many irritants produce no contraction, but cause dilatation as the primary phenomenon, and all irritants do so if the nerves are cut before their application. The very terms "irritant" and "irritation" (Prof. Hamilton uses both) as applied to the phenomena of inflammation, imply a paralysing as well as a stimulating action on the vessels; and if this be so, then the greater the intensity or the more prolonged the action of the irritant, the more rapidly will the dilated or paralysed state of the vessels ensue, we should say in either case, before the contraction has lasted long enough to cause capillary stagnation. Again, in speaking of spasm of the pulmonary arteries, as is done at page 115, it should be remembered that one of the peculiarities of the pulmonary arteries, as compared with the other arteries of the body, is that they possess but little tonicity.

Professor Hamilton, too, thinks that "pathologists have run mad over this 'exudation-of-leucocytes' theory," and he believes that they "have mistaken its true nature." In this we are not at all prepared to agree with the author, who "can see nothing more in the exudation of a solid material than the action of increased, and, more especially, suddenly increased, pressure upon a fluid holding these solids in solution, and confined within an animal membrane of great delicacy." If simple mechanical increase of pressure be the primary and chief cause of the exudation of leucocytes, we would expect that the exudation would be greatest in the arteries, where the pressure is greatest, because they are situated behind the capillaries, in which the obstruction or stasis, as Professor Hamilton points out, occurs. But it is not so. Cohnheim has shown, and we have verified it by personal observation, that exudation only occurs in the capillaries and veins, where the pressure is least. The author endeavours to prove his opinion by quoting some experiments which he has carried out, in which pieces of gelatine were extruded through very small apertures in the membrane of a dialyser and in the walls of a perforated tube, by increasing the pressure or obstructing the outflow; but the purely mechanical nature of these conditions renders them incapable in many respects of being compared to what takes place in a vital structure like a blood-vessel, where phenomena above and beyond the merely mechanical come into play. Besides, such an apparatus as this can, we think, only be strictly compared to an artery in which, as we have seen, no exudation of leucocytes occurs.

We are astonished, too, to find Professor Hamilton writing in the following strain:—"It is said that the cause of their exuding is that they possess amœboid movements, by which they are enabled to insinuate themselves into the natural openings or stigmata between the endothelial cells, and gradually to push their entire body outwards. What the exact reason is for this somewhat extraordinary behaviour of the leucocytes has never been clearly explained, but it is generally assumed that the 'inflamed tissue' has some special attraction for them, which induces them to leave their natural habitat. This is entirely a matter of theory, to which experiment has not given the slightest support." It is well known that leucocytes, if brought into contact with unhealthy or dead material, are excited to amœboid movement and become adhesive. Professor Hamilton is surely aware of the observations long ago made by Recklinghausen, that when the dead cornea is introduced into the lymph sac of the frog, leucocytes penetrate into its substance in large numbers.

We think that this article upon inflammation is, perhaps, the most unsatisfactory in the whole book. No doubt, if one can rest satisfied with them, such doctrines concerning the pathology of inflammation will be more easily remembered and understood than the more comprehensive, and, in our opinion, more scientific views of Cohnheim and his school. To our mind also, they seem far removed in spirit from the enduring tone of John Hunter's works, in which many protests occur against regarding the animal body as a mere piece of mechanism, and leaving out of view the fact that it is endowed with life in every smallest part.

In the articles on catarrhal pneumonia and tubercle in the human lung, amongst much careful anatomical description, there are also many points calling for criticism, but our space will only permit us to refer briefly to a few of these. Professor Hamilton is a strenuous advocate of "the doctrines and logical inferences of Virchow and Niemeyer," and believes that tubercle in the lung "is invariably preceded by some source of infection, usually caseous deposit." At another part of the book the following occurs:—"There is, and I say it unreservedly, in those cases of so called isolated tubercular meningitis, invariably, either in the brain substance, the bone, or the encephalic membranes, or some other part, a cheesy deposit, which is *not* primarily tubercular, but which usually has an ordinary inflammatory origin: it is this which acts as the infecting centre and which induces the tubercular meningitis. According to Cohnheim, however, this mere caseous mass is regarded as being throughout of the same nature as the tubercles which result from it." We do not intend to criticise these statements further than to remark that by many competent authorities they are now entirely set aside. Tubercle in the lung is divided by the author into primary and secondary, and we think that the basis of the distinction is rather unscientific. "To the former, that is to say, where the caseous source of infection is in some distant part or organ, and where the tubercles alone are present in the lung, I shall give the designation of 'primary tubercle.' To the latter, where the tubercle nodules and the caseous source of infection are together present in the lung, I shall give the name of 'secondary tubercle.' In relation to the lung, the tubercle in the former is the primary, and, in fact, usually the only disease. In the latter the caseation, from whatever cause arising, is the primary disease, and the tubercle is consequently secondary." This may be a convenient, but it is certainly not a very scientific distinction.

The last article in the book, on "The Supposed Contagiosity of Tuberculosis and Pulmonary Phthisis," is devoted to a criticism of the conclusions drawn from Koch's discovery of the tubercle bacillus. In reading this portion of the work, one is impressed with the idea that there is a struggle, and a pretty severe one, going on in the author's mind between the views which are so strenuously argued in the course of the book and the influences which later discoveries have brought to bear on them. We cannot consider the question at present in greater detail; but speaking for ourselves we must conclude by saying that we do not think Professor Hamilton has succeeded in proving his point.

While we have felt compelled to disagree with Professor Hamilton in many of the opinions here stated, we are able to express our high estimation of the book as representing a large amount of careful work which will have an enduring influence on the pathology of the subjects dealt with. The book also shows the merits of Professor Hamilton as a teacher, although we cannot shut our eyes to the fact that his dogmatism can hardly fail to exercise on the undeveloped mind an influence which is apt to be one-sided.

The Filaria Sanguinis Hominis, and Certain New Forms of Parasitic Disease in India, China, and Warm Countries. By Patrick Manson, M.D., C.M., Amoy, China. London: H. K. Lewis. 1883.

It is a great advantage to have the admirable observations of this author in an accessible form, instead of being hidden in the *China Customs Gazette*, which the author correctly calls "a practically inaccessible periodical." The general life-history of the filaria sanguinis was fully considered in a paper communicated by the author to the Pathological Society, and published in their *Transactions* for 1881. This paper is reprinted here, and we have various others added.

One of the most interesting points in the career of this little parasite is that the embryo form which occurs in the blood shows a very striking periodicity. It is absent during the day, begins to appear in the evening, and attains its maximum prevalence in the blood about midnight. The nocturnal habits are related to the fact first observed by our author, that the intermediary host is the mosquito, a nocturnal animal, so that this periodicity is a provision by which the preservation of the species is effected. Writers are not agreed as to the explanation of this periodicity. Some hold the view that the female gives birth every evening to a multitude of embryoes, which live through the night and then die. This would imply an enormous fecundity on the part of the female, as in some cases 100 or more embryoes are contained in every drop of blood. The other view, which seems a much more probable one, is that during the day the embryoes retire to some part of the circulation and remain there at rest. In order to assist in deciding this question, the author had recourse to the lower animals. Dogs are frequently the hosts of a filaria, of a different species to the human one, and called the filaria immitis. This parasite does not show such a degree of periodicity as the human one, but in the case of two animals there was a certain daily diminution. He killed the animals when the number was comparatively small in the blood drawn from the skin, and found that in the blood from the lungs the filaria was present in very much larger numbers than in the other organs, the numbers being for each drop of blood taken, in one case, as follows:—Ear, before death, 82; liver, 324; lungs, 4,582 in one drop, 1,591 in another, and 2,738 in a third. It seems legitimate to infer that in man the embryoes probably betake themselves to the vessels of the lungs or liver during the day, and swarm out at night.

A question, concerning which considerable discussion has taken place, is here somewhat fully dealt with—namely, the relation of filaria to elephantiasis. The author regards elephantiasis as due to the parasite, and cites in evidence cases in which lymph scrotum, which undoubtedly depends on the filaria, has passed into elephantiasis; another in which lymph scrotum followed operation for elephantiasis, and so on. There are several objections to this view, and it seems to us to be hardly supported by the evidence adduced. In lymph scrotum it is common to find ova or embryoes of the filaria in the fluid which drains off; but we fail to find, in these cases or elsewhere, any mention of their presence in fluid removed from parts the seat of elephantiasis. Then although he says that in many cases of elephantiasis the previous history is that of lymph scrotum, he does not in the least indicate the proportion in which this is true. He states that the numbers are so great that it could not be a mere coincidence. We would venture to point out that it might not be a coincidence and yet be susceptible of another interpretation than that which the author gives. If elephantiasis is not due to the filaria, it is probably referrible to the action of some micro-organism, taking rank along with leprosy in this respect. If this micro-organism is present, it will find access much more readily in cases of lymph scrotum where the skin is broken and the lymphatics exposed, than in persons whose skin is intact.

Of the other parasites described here, the most important is the distoma Ringeri. This is a fluke which inhabits the lung, and sheds large numbers of ova. The result is hæmoptysis, and the ova are present in large numbers in the expectoration. The book ends with a description of the skin disease tinea imbricata, and the author insists on its essential difference from tinea circinata. He gives drawings of the fungus, and relates experiments in which the disease in all its characters was propagated from one person to another.

L'Année Médicale, 1882. By DR. BOURNEVILLE, and Others. Paris: E. Plon et Cie. 1883.

THIS annual, one of the most useful of its kind, has now reached its fifth year of publication. It gives, within the compass of about 450 pages, the essence of all that has been accomplished in the way of the advancement of medical science in 1882. Its articles are short, concise, and apparently better digested than in some other medical annuals. The preparation of the work is carried on by Dr. Bourneville and a large staff of co-workers, whose names are a guarantee of the excellence of the *résumé* they offer. It is the case that the papers and books noticed in its pages are mainly Continental, but this does not detract from its great value, but in some respects rather adds to it, for the English reader.

Not the least interesting section is that devoted to the record of those who have "fallen victims to professional duty" in 1882. Beginning with France, and taking up other countries in detail, the names of the most prominent members of the profession who died in that year are given, together with a short notice of their principal works and achievements. Among those honourably mentioned we find the name of Dr. D. Foulis.

We repeat our commendation of this volume, and advise those who wish to possess a work of the kind to think of *L'Année Médicale.*

The Extra Pharmacopœia of Unofficial Drugs. By WILLIAM MARTINDALE, F.C.S., and W. WYNN WESTCOTT, M.B. Lond. London: H. K. Lewis. 1883.

MOST striking evidence of the immense progress made in the science of Therapeutics during the past ten years will be found in the pages of this work, especially if it be compared with our present official pharmacopœia. When it is borne in mind that since the issue of the last edition of the British Pharmacopœia, in 1874, such drugs as salicin, salicylic acid and the salicylates, chrysophanic acid, jaborandi and pilocarpine, iodoform, eucalyptus, nitro-glycerine, the oleates, vaseline, thymol, have come into every-day use, it will be admitted that during no previous decade have more important additions been made to our armamentarium. The revision of our pharmacopœia, expected next year, is urgently called for. There are many drugs which might with advantage be dropped from our present list, while on the other hand there is little doubt that many of those described in the book before us will obtain admission to its pages. In view of the fact that so many of these newer drugs are extensively employed by all practitioners, no more valuable reference volume of the more recent materia medica could be imagined than that under review. It seems to be complete, no newer drug having any claim to consideration being omitted. Mr. Martindale's name is enough to warrant implicit faith in its accuracy and fulness. Its value to the physician is much increased by the remarks of Mr. Westcott on the uses to which the drugs mentioned have been put, all such notices being guaranteed by quotation of the source of information in the medical journals and in text books.

The only hint of inaccuracy we would offer is with reference to the dose of sodium nitrite, stated here as 5 to 12 grains. Dr. Murrell's recent experience, regarding which there has been so much unpleasant discussion in the lay and medical press, shows that in administering a *pure* sample of this drug it is safer to commence with a smaller dose, not more than 3 grains.

We consider this book, especially in the present state of our authorised pharmacopœia, an indispensable consulting-table companion to all those who wish to keep abreast of the times in materia medica. We believe its success has been so great that a second edition has already been called for.

Since the above was set up in type, the second edition has come to hand. It differs from the first only in containing reference to a few more drugs (23), and a therapeutic index. These will doubtless add to its value.

Transfusion: its History, Indications, and Modes of Application. By Chas. Egerton Jennings, L.R.C.P., London. Pp. 69. London: Baillière, Tindall & Cox. 1883.

Impressed with the many difficulties and dangers which surround the operation of mediate or immediate transfusion, Mr. Jennings has in this brochure made a laudable attempt to show how the operation may be simplified and its dangers obviated. He believes that many women are annually lost from the difficulty experienced by the practitioner of obtaining a blood donor and the skilled assistance necessary to enable him, perhaps in the middle of the night, and in a lone country house, to perform such a delicate operation. Still, as the statistics published by Roussel and others show that more than half the total number of cases of transfusion recover, he considers the operation not only permissible, but even obligatory on the practitioner "whenever acute anæmia of sufficient severity to threaten life sets in as the result of hæmorrhage." After reviewing the present methods of operating, and pointing out their defects, he proposes the following:—A slightly curved, blunt pointed canula is inserted into the vein of the recipient, and to this is attached a tube five feet long, with a stop cock at the proximal and a syphon at the distal end. The whole apparatus is very like the common nasal douche, and is used in the same way. By this means a constant stream of a saline alcoholic fluid is easily introduced, and all necessity for skilled assistance dispensed with. The formula of the fluid preferred by Jennings is:—

Chloride of Sodium, . . .	50 grains.
Chloride of Potassium, . . .	3 ,,
Sulphate of Soda, ...	2·5 ,,
Carbonate of Soda, . . .	2·5 ,,
Phosphate of Soda ($Na_3 PO_4$), .	2 ,,

This is dissolved in 20 ounces of water at 100° Fahr., and 2 drachms of absolute alcohol added.

If it be thought desirable to introduce blood this can be done at the same time by inserting a Y shaped glass tube in the india rubber tubing, and attaching another tube from the donor to the branch. By means of properly arranged stopcocks also, if the donor should faint, the stream of saline fluid, without the slightest displacement, may be at once turned into his veins, and so the blood drawn replaced.

The chief indications for the operation and its mode of performance are stated with a brevity and clearness which ought to recommend the book to the busiest practitioner, while a very complete Bibliographical record is added for the use of those who wish to investigate the literature of the subject.

Mechanical Exercise a Means of Cure: being a Description of the Zander Institute, London. Pp. 92. J. & A. Churchhill. 1883.

The value of regulated physical exercise as a means of increasing the force and tone of muscles, of restoring stiffened joints, and of preventing many of the inconveniences of sedentary habits has long been familiarly known. Its application, however, has in most instances been left to the people themselves, who have been guided in the matter too often by their own fancies or the fashion of the day. Skilled persons, medical or otherwise, beyond taking a general interest in the training of athletes, or recommending a few gymnastic exercises to young people, have very seldom, in this country at least, taken much thought as to how the therapeutic benefits of exercise were to be brought within the reach of ladies, elderly men, or weakly persons of all ages and of either sex. To meet this difficulty, Dr. Zander of Stockholm has invented some most ingenious machines whereby the several muscles and joints of the body can be brought successively into play with perfect safety to the most confirmed invalid or the most delicate child.

A complete set of these machines has now been introduced into London (7 Soho Square), and in this little book we find a full description of the method, and of the principal machines and their uses. Every form of movement can be provided for by a machine with specially arranged levers and weights. A stiff joint is acted on by one machine, a weak joint by another. There are machines for rotation, or flexion, or extension of the spine; for stimulating the muscular walls of the intestine, or oscillating the larynx; for kneading, rubbing, or percussion; for the treatment of rheumatism, sciatica, sleeplessness, heart disease, dyspepsia, constipation, or atony of the bladder. Wherever, in fact, muscular tissue exists, there, according to Zander, one of his machines may be applied with benefit.

We heartily wish that this "movement cure" should become more widely known, even if it should not prove to have all the therapeutic value its promoter claims for it. Its virtues ought to be tried, for instance, in lateral curvature of the spine, which we think surgeons at present are treating too much by fixation. Regulated exercise may possibly replace all forms of mechanical support in such cases. It is not likely, however, that Zander's method in its entirety will ever become very popular among us. It is too complicated and too costly, and we fear would require for its proper development a limited liability company with considerable capital, faith, and patience.

Observations on Lithotomy, Lithotrity, &c. By Reginald Harrison, F.R.C.S., Liverpool. Pp. 71. J. & A. Churchill. 1883.

Mr. Harrison has gathered together in this pamphlet some most interesting observations on the operative treatment of stone in the bladder, derived from his experience of over sixty cases. It is difficult to say anything new about lithotomy, an operation which he practically confesses was perfected by Cheselden, but certainly no one has stated the common things better or with more attention to the perspective of the subject than Mr. Harrison.

As regards lithotrity, Mr. Harrison is an enthusiastic disciple of Bigelow. Being in America he saw Bigelow operate in April, 1878, and he returned to this country fully impressed with the value of the new operation. He brought Bigelow's apparatus with him, and has since performed the operation 28 times. He thinks Bigelow's operation destined to replace Civiale's altogether, and even to reduce the number of cases hitherto submitted to lithotomy.

Elements of Histology. By E. Klein, M.D. London: Cassell & Co., Limited. 1883.

This little work constitutes the first of a series of medical manuals at present in course of publication by the well known London firm, Messrs. Cassell & Co., Limited. In the department of histology anything associated with the name of Klein is generally expected to be good, and in the present instance we must congratulate both the author and the publishers upon the excellence of the work which they have produced, and which promises well for succeeding manuals. The work is intended for medical students, and we think that they will not be slow to avail themselves of the advantages to be derived from the possession of such a volume. It will not frighten the beginner with its size and weight, as some of his other tomes are apt to do, and, when he comes to study it, he will find there is far more in it than he would have supposed on comparing it with larger books. The arrangement adopted is good and simple, and the descriptions, though necessarily somewhat short, are by no means wanting in perspicuity and force. Perhaps the most pleasing feature of the whole book is the wealth of beautiful illustrations in which it abounds, and, when it is stated that the great majority of them are taken from the author's *Atlas of Histology* or from the *Handbook for the Physiological Laboratory*, any one who knows anything of these works will easily understand that the woodcuts in the present manual are excellent of their kind, and that they will prove of great service to the student.

We do not know any work of the same size and price which so nearly approaches our ideal of a manual of histology for students, and we heartily wish it success.

The Physiological Factor in Diagnosis. A work for young practitioners. By J. MILNER FOTHERGILL, M.D. London: Baillière, Tindall & Cox. 1883.

WE hasten to relieve the anxieties of our readers by giving them at once the last paragraph of this book:—

"And now the time has arrived when the writer, as a medical author, must take leave of his readers. He has written voluminously; not in vain, if the sale of his works can be trusted as any valid evidence of the favour they have met with. The encouragement so given him has incited him to go on; despite the impression abroad that much writing means little practice. His books have brought him practice, each its modicum. Nevertheless, for the future, his time must be devoted to the revision and enlargement of already existent works, in the new editions their sales demand. It is, indeed, a pleasure to write for appreciative readers—that pleasure he has tasted liberally. *Vale!*"

This quotation shows the author to be well satisfied with himself—his books selling and his practice increasing: even reviewers are put into good humour by the hope that he will bring out no more new books, and that his time may now be given to improving, or at least to revising and enlarging, as he says, new editions.

Thus reassured, we are glad to admit that there are many good points in this book; indeed, we begin to hope for something from the "revision and enlargement" referred to; for we find that this book may, to a great extent, be regarded as an old friend with a new face—viz., his little book on *Aids to Diagnosis—Semeiology*, formerly noticed in this *Journal*. We think that in this new form the writer has managed to improve his style and to make his work more useful.

The volume contains chapters on the external appearance, the tongue, respiration, pulse, alimentary canal, urine, &c., and the indications to be drawn from them are described, or at least indicated, as he goes on.

A Handbook of Therapeutics. By SYDNEY RINGER, M.D., Professor of the Principles and Practice of Medicine in University College; Physician to University College Hospital. Tenth edition. London: H. K. Lewis. 1883.

THE later editions of this well known Handbook have been very materially improved by the more general chapters prefixed to the work, on various important indications of the diseased condition; and on certain therapeutic measures of a general kind, such as the external use of hot and cold water, acupuncture, counter irritation, &c.

One turns with some curiosity and anxiety to the passage in this edition on nitrite of sodium, the use or abuse of which has of late been so prominently discussed in the medical and general papers, and we are glad to find that the recommendation of the author himself does not go beyond "one grain repeated several times a day."

The dietary at the end of the book and the index to diseases, with an analysis of the remedies recommended in the text, continue in this edition to add much to the value of the work.

Royat (Les Bains), in Auvergne: its Mineral Waters and Climate. By G. H. Brandt, M.D. Second Edition. London: H. K. Lewis. 1883.

Hammam Rirha, Algiers: a Winter Health Resort and Mineral Water Cure combined. By G. H. Brandt, M.D. London: H. K. Lewis. 1883.

Those who are interested in knowing something of the mineral waters of Auvergne, and the advantages to be derived by rheumatic patients from a residence in Algiers, will find much useful and practical information in the above little works, which are suitable for the general as well as the medical reader. The waters of Royat abound in alkaline salts, and seem to be specially beneficial in the treatment of gout and allied affections. The following quotation from Dr. Brandt will give some idea of the general composition and nature of the springs:—"The four mineral springs at Royat, although of the same type—chloro-alkaline—differ as to their mineralisation and their temperature: the presence of chloride of sodium and other salts shows a marked difference from the purely alkaline waters, of which we might quote as types Vals and Vichy.

"The salts of soda, potash, lime, and lithia represent in these waters the alkaline elements, the depressing effects of which are counteracted by the tonifying properties of the salts of iron and arsenic which, combined with the stimulating effects of their carbonic acid, render them so powerful and active."

Hamman Rirha in Algeria is not yet so well known in England, but has been visited and described by Mr. G. D. Pollock, F.R.C.S.E., and Dr. Lauder Brunton. The climate and scenery are good, and there are two springs—a hot spring, of which the most important ingredient is sulphate of lime, and a cold chalybeate spring. "The diseases in which these baths have been shown by actual experience to be useful are chronic rheumatism, articular or muscular, gout, stiff tendons, erratic pains, and neuralgias of rheumatic origin."

Travellers and patients proposing to visit either spring will find all necessary information in Dr. Brandt's little brochures.

A Practical Treatise on Electro-Diagnosis in Diseases of the Nervous System. By A. HUGHES BENNETT, M.D., Physician to the Hospital for Epilepsy and Paralysis, Regent's Park, and Assistant Physician to the Westminster Hospital. London: H. K. Lewis. 1882.

THE use of electricity in the treatment of disease is now so general that a much wider circle of readers may now be expected to take an interest in such a work as this. The importance of the electrical reactions is recognised by all those who have studied the subject, not only as regards diagnosis, but also as governing the prognosis, and as guiding the treatment. The information on this interesting subject is scattered through many special works on electro-therapeutics and nervous diseases, and the subject is not always approached from the point of view of supplying full and detailed information on the methods pursued in applying the tests. We are glad, therefore, to be able to recommend this book to those of our readers who are in search of a full statement of the subject, with all necessary details and illustrations.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM PROFESSOR BUCHANAN'S WARDS.

CASE OF ENLARGED CASEOUS GLAND, SIMULATING A MAMMARY TUMOUR.—[Reported by Mr. Stafford.]—Mrs. F——, æt. 30, housewife, was admitted upon the 10th November, 1883, to Ward VIII, on account of a tumour in the left breast, of which the following is the clinical history:—While suckling her second child, 4 years ago, she experienced a wearied sensation in the left arm, but besides this, at that time, neither swelling nor disordered sensation of the parts was complained of. Soon after the appearance of this symptom, a tumour was accidentally discovered in the left axilla at its anterior aspect. It was painless, and did not interfere with lactation. When first noticed it was about the size of a walnut, and it slowly increased in size, being on admission about the dimensions of a goose egg. She states that the tumour gradually went lower down the chest, following, apparently, the outer edge of the pectoralis major, and ultimately resting upon the upper and outer aspect of the mamma.

The tumour, on examination, was found to be circumscribed, without adhesion to neighbouring parts, and the skin covering it was unaltered in appearance. There were no enlarged glands in the axilla, but the entire breast was much larger than the right one. The enlargement of the organ has been present since the time of the first lactation.

The patient herself is a stout, florid woman, who has always enjoyed good health; she has had three children, two of whom died during dentition. She has never suffered from enlarged glands elsewhere, and none of her relations have ever done so.

16th November, 1883.—An incision was made through the skin, and the tumour removed by enucleation.

The following is Dr. Joseph Coats' report upon the microscopical and naked eye characters of the tumour:—"Two defined masses are sent for examination, one the size of a lymphatic gland, and the other that of a small apple. The larger consists partly of adipose tissue, in which are one or two more solid nodules; and partly of a structure which is circumferentially solid, but internally consists of a pultaceous material. This part has a striking resemblance to a strumous testicle. Under the microscope the smaller tumour, as well as the solid part of the layer and the nodules in the fat of the latter, all present the same structure. They are mainly caseous, but at their marginal parts show more or less round cells. In one place where these were more abundant, a giant cell is found."

From Dr. Tennent's Wards.

CASE OF GASTRIC ULCER PERFORATING A BRANCH OF THE CORONARY ARTERY, AND CAUSING DEATH FROM ANÆMIA DUE TO HÆMORRHAGE.—[Reported by Mr. Skottowe.]—A. M., æt. 69.

On admission, the patient was exceedingly stupid and delirious, and it was with the greatest difficulty that he could be got to give an intelligent answer to any question regarding his condition. On particular enquiry as to pain and the seat of it, he only said he was very ill, and his hand wandered over the abdomen as if to indicate that the pain was there; but he could not localise any spot as being more painful than another, and pressure all over the belly did not seem to increase it. On further examination of the abdomen, no tumour or swelling could be detected; there was no tympanites, and both inguinal rings were quite free. Shortly after admission he vomited a little "coffee ground" material, and he also had a little hiccough, but both these symptoms soon passed off. The mouth and lips were exceedingly anæmic; the right pupil was a little more dilated than the left, but both responded to light. The only account obtained about the patient, on admission, was that he had been ill for about three weeks, and that about the beginning of the illness he had vomited blood. The pulse was 96, soft and feeble; the temperature was 97°; the urine was pale; sp. gr. 1015; reaction acid, no albumen. There was no alteration of muscular power, and the bowels were not moved for some time after admission.

6th December, 1883.—This morning Dr. Tennent made the following note of this patient's condition:—The pulse is 116, very soft and feeble; the radials seem fairly straight, and there is no unusual prominence of the temporals. The respiration is 16, and the temperature 97·6° at 9 A.M. He is presently lying in a condition of considerable stupor, accompanied by restlessness, constantly tending to wave his arms and hands over his face and body generally, and talking in an incoherent way; but he replies, when loudly spoken to, that his name is Alexander M'Indoe; and when again spoken to, at once asks, "What do you say, sir?" The pupils are equal, though somewhat dilated, but they readily respond to a lighted taper. The face—lips in particular—and the whole cutaneous surface, are extremely pale; the lips, indeed, are literally blanched. The gums, too, and the whole lining membrane of the cheek and fauces, are also absolutely blanched, as is also the tongue, so far as can be seen; for when the patient is asked to put it out, he only does so very imperfectly, and even that only after being repeatedly spoken to. Distinct pulsation is seen in the superficial veins on the right side of the neck, but this seems to be somewhat transitory, as during examination it ceased and again recurred. Percussion of the lungs in front is normal, and the cardiac percussion is also normal. Auscultation all over the chest in front shows the respiratory murmur to be of good volume and quality, and equal on both sides. The apex impulse is distinctly felt in the fifth interspace, but varies greatly in its intensity. It is almost directly beneath the nipple, and is very limited in area. Percussion of the back is also good, and nothing is elicited on auscultation, except that the respiration is, if anything, a little feeble at the left side. The cardiac sounds seem pure, except that there is a little prolongation of the first sound, heard especially over the lower end of the sternum. As the heart sounds are feeble and the respiration comparatively full, it is somewhat difficult to hear them with sufficient distinctness. The areas of hepatic and splenic dulness seem normal ; and very careful examination of the abdomen fails to detect any evidence of tumour or unusual hardness of any kind, but accurate examination is much interfered with by the marked rigidity of the abdominal muscles. There is no special fulness or hardness to be felt in either lumbar region. During examination the patient has been somewhat delirious and excited, but becomes more so during examination of the abdomen. Careful examination detects nothing abnormal in the inguinal regions.

7th December, 1883.—Towards yesterday afternoon, this patient became quite unconscious, and as he was unable to swallow, he was fed by enemata. For this purpose the lower bowel was emptied by a soap and water enema, which brought away *a large quantity of dark, tarry-looking material.* The nutrient enema was then given about 5 P.M., and retained. Another was administered at 9 P.M., and a third early this morning. He has had no recurrence of the vomiting. Temperature in the rectum, 101° ; in the axilla, 99·4° ; pulse, 112. This afternoon the bowels were again moved by injection, followed by a nutrient enema, which was retained. He lay in an unconscious moaning state all afternoon, and died about 8 P.M.

The following is Dr. Joseph Coats' account of the *post mortem* examination :—

External Appearances.—The body is fairly well nourished, but there is general pallor of the skin.

Chest.—The heart is very large, weighing 19¼ ounces. The enlargement is mainly from hypertrophy of the left ventricle. The valves are normal, and, in particular, the aortic was found perfectly competent, as tested with water before opening. The muscular tissue is remarkably pale, and perhaps unduly opaque, but with very little of the flecked appearance usual in fatty degeneration. The lungs are non-adherent, and are highly œdematous posteriorly.

Abdomen.—The spleen is normal, weighing 4 ounces. The kidneys are normal in appearance, weighing $4\frac{1}{4}$ and 4 ounces. On opening the stomach, two ulcers are found—a large one of an irregularly oval shape, measuring $1\frac{3}{4}$ in. in its long diameter, and $\frac{5}{8}$ in. across. It is situated on the anterior wall, with its edge just touching the lesser curvature, at a point $1\frac{1}{2}$ inches from the pylorus, its long diameter extending at right angles from the curvature. The small ulcer is round, and $\frac{3}{16}$ in. in diameter. It is situated on the posterior wall, at a point opposite the extremity of the large ulcer farthest removed from the lesser curvature. Both ulcers have the punched out appearance characteristic of the perforating ulcer. In the floor of the larger one there is an elongated elevation running nearly parallel to its long diameter. An aperture is found in this, into which a bristle can be passed, and the elevation is determined to be a branch of the left superior coronary artery, which the ulcer has laid open. The liver is pale, and weighs 2 lbs. 14 ounces.

Microscopic Appearances.—The muscular substance of the heart was found to present a generalised fatty degeneration of a somewhat advanced degree, and this was present in the walls of both ventricles, with apparent uniformity. The liver also showed fatty degeneration, the hepatic cells being almost uniformly occupied by small fat globules, which varied little in size.

PRIVATE PRACTICE.

Case of Tape Worm Treated by Tannate of Pelletierine.

By Mr. J. B. LAWSON, M.B., C.M.

P. M'J., æt. 43, labourer, consulted me on the 28th October, 1883. He had been for more than five years the victim of tape worm, and during that time he had submitted himself to the skill of several medical men, and tried many remedies. Large doses of the extract of male fern and kamala had frequently given him considerable relief, and as the result of their action he had passed at one time 19 feet, and at another 22 feet of the parasite. Considering that the common remedies had been tried so often, and had just as often failed to extirpate the worm, I thought the case a good one for trying the virtues of tannate of pelletierine. The patient having prepared himself by fasting for some hours, I prescribed 6 grs. of the tannate of pelletierine; this was to be taken early in the morning, and followed shortly afterwards by a large dose of castor oil. A fortnight afterwards the patient returned to announce the failure of the drug. He had passed no more of the worm than he had often done as the result of a dose of castor oil, and from his sensations he believed that the worm had not been at all affected by the drug. Fearing that some of the instructions had not been carried out, or that some of the powder had been lost, I asked for another trial of the drug, and the patient readily acquiesced from his anxiety to be freed from so troublesome a companion. This time I prescribed 7 grains, to be taken early in the morning, and followed a quarter of an hour after by a large dose of castor oil, the supper the previous evening to consist of bread and milk. A fortnight after he returned again to state that the result was the same as in the former instance—only a few bits of the parasite had come away, and he was not in the least relieved. That was sufficient for me; tannate of pelletierine had failed, and I at once fell back on the proved remedies of the pharmacopœia. I now prescribed as follows:—

℞.

Extracti filicis maris ʒiss.
Pulveris kamalæ ʒij.
Mucilaginis acaciæ.
Syrupi simplicis āā ʒij.
Aquæ cinnamomi ad ℥iij.

M.

Sig.—Half to be taken at bedtime, and the other half early in the morning.

I was gratified to learn that it had been effectual; fourteen feet of the worm had been passed, and the patient's uncomfortable sensations greatly alleviated. It would be rash to predict anything but a temporary relief, as the patient had to defæcate outside and under circumstances that rendered it impossible to search for the head of the parasite.

Tannate of pelletierine is a drug difficult to be had; it is very expensive (4s. a scruple, wholesale), not to speak of its liability to adulteration; and then it has a disadvantage which, I think, attaches to all powders—viz., that you can never be sure that the whole of it has been taken by your patient unless you are present, as it is so liable to be lost in the mixing and swallowing.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY.

Session 1883-4. Meeting II.—*2nd November, 1883.*

Dr. Gairdner, *President, in the Chair.*

Dr. Robert Kirk, Partick, read on myxœdema, and showed a patient suffering from that disease. (See page 1.)

Dr. Alexander Robertson said that, after carefully examining the case, he coincided with Dr. Kirk in regarding it as one of myxœdema, though differing slightly from the description given by Dr. Ord, more especially in there being no defect in the special senses. With regard to the integument, especially that of the hands, it appeared to be very attenuated. This case appeared also to have lasted longer than other recorded cases. He had seen two cases which, looking back on them now, he could identify as probably of the same kind. Indeed, he at first thought that Dr. Kirk's patient was one of these, but found that he was mistaken.

Dr. Morton said that during the past summer he had seen a case of the kind in one of the central towns of England, but without recognising it at the time. He was struck in that case with the doughy yellow appearance of the skin and with the thickened condition, not only of the fingers, but of other parts of the body, especially those about the buttocks.

The President said that one result of the exhibition of this case would be to put them on the outlook for similar cases. Not long ago he had, in consultation with Dr. Mather, seen a case which impressed him as one of myxœdema, though he was not absolutely certain.

Dr. M'Vail said that more than ten years ago, and before the appearance of the first observations on this affection, he had seen in the house of a medical man a thoroughly typical case, but without identifying it as a special disease. The patient, a lady of about 60, had been in her youth the beauty of the district, and a rapid speaker. There was very great œdema of the cellular tissue all over the body. The skin of the eyelids had swelled out and looked like jelly, and there was thickening of all the features. She spoke with extreme slowness, every word being articulated so as to be detached from the next. There was also the characteristic injection of the vessels of the cheek. Her movements were executed with the utmost deliberation, and he attended her for a fracture of the leg, caused by her falling over some slight obstacle in the room. There was no albumen in the urine, and no trace of any kidney affection. He left the district before her death, and did not know the cause of death.

Dr. Kirk said that he had omitted to speak of several points, such as the laughter of the patient, which was only a prolonged smile, with a feeble sound at the end. In regard to the special senses he failed to remember any remark of Dr. Ord as to their being defective. On the contrary, he stated in some of his cases that all the special senses were perfect. They would have observed the slowness of the response to impressions; and it was noticeable that this slowness was greater at some times than others. The thinness of the skin, mentioned by Dr. Robertson, raised the point what was to be reckoned skin. The connective tissue was much increased.

PATHOLOGICAL AND CLINICAL SOCIETY.

Session 1883-84.

Meeting III.—*11th December, 1883.*

The President, Prof. M'Call Anderson, *in the Chair.*

Dr. Joseph Coats showed a portion of the stomach with a large and a small simple ulcer, which were found in the usual situation of such lesions, the large one being near the lesser curvature and extending somewhat on to the anterior surface. Death had resulted in this case from a hæmorrhage, occurring three weeks previously to the fatal issue, and in connection with this an artery was discovered in the floor of the ulcer with an open mouth, into which a bristle could be readily passed. The heart and liver from the same case were shown. The heart was pale, and presented the most pronounced fatty degeneration of both the right and the left ventricles. The liver was very pale, and presented practically the same condition as was met with in the heart—viz., a fatty degeneration of the hepatic cells. For contrast with this case an example of fatty infiltration of the liver was shown, in which the fat globules, as seen by the microscope, were much larger and coarser than they were in the case due to hæmorrhage.

Dr. Gairdner asked on what grounds Dr. Coats supposed that the whole of the fatty degeneration had occurred since the hæmorrhage.

Dr. Coats replied that it had been proved by experiment upon dogs that fatty degenerations, such as he had described, could occur very rapidly after artificial bleeding, and referred to another case of his own where a hæmorrhage from the stomach, three weeks before death, had produced similar appearances. In this case the ulcer had healed between the occurrence of the bleeding and the period of death.

Dr. Coats also showed a well marked example of POST-MORTEM DIGESTION OF THE WALLS OF THE STOMACH.

Mr. Maylard exhibited the URINARY APPARATUS from a case of STRICTURE OF THE URETHRA, and read the following notes of and remarks concerning it:—

This specimen was removed from a case of old standing stricture, and the preparation, as a whole, is a very good illustration of the changes resulting from long continued obstruction in the urethra.

In the penile portion of the canal is a very tight and tortuous stricture, in front of which are numerous false passages here represented by small black directors. Behind the stricture is the greatly distended membranous portion of the urethra. The broad, flat, black director passing into it denotes the spot at which it was incised. In connection with the prostate is a large abscess. The bladder is much enlarged, and its wall enormously hypertrophied. There is dilatation of both ureters and commencing double hydronephrosis. To the naked eye the kidney structures did not appear abnormal.

My reason for bringing the specimen before the Society is not so much for anything very remarkable in it, but because of the difficulty, both as to diagnosis and treatment, which the extremely hypertrophied bladder gave rise to.

The clinical features of the case are briefly the following:—The patient was admitted into Ward XVIII of the Western Infirmary suffering from retention of urine. The report states that "he is very broken down and emaciated, and seems half stupid. He says he has stricture of the urethra, and for a long time has had difficulty in passing his water. Four days ago he went to the Royal Infirmary to get his urine drawn off; but as no catheter could be passed he was aspirated above the symphysis pubis. The next day he returned, and it is said that a No. 2 was passed (this, however, was carefully inquired into, but nothing definite could be ascertained.) To-day he again went to the Royal Infirmary, and as no catheter could be passed he was again aspirated above the pubis." This, so far, is the account given by the patient, and must, therefore, be guardedly received. Four hours later —that is, as he states after being tapped at the Royal Infirmary—he was admitted into the Western Infirmary with his bladder, according to the house surgeon's statement, distended up to the umbilicus. Looking at the bladder I feel some doubt as to this being really possible; but the condition was supported by other observers, and I would more readily accept what to them existed as a fact than what to me only exists as a conjecture. Attempts were made to pass a catheter, but failing, chloroform was administered, and again all endeavours to enter the bladder were ineffectual. I was then sent for and found the patient in a drowsy condition, apparently, as I supposed, not yet recovered from the effects of the chloroform. There was dulness for about a couple of inches above the pubes. I tried to pass catheters, and failing to do so I deemed it advisable, considering the amount of catheterisation he had already undergone, to puncture above the pubes in preference to a prolonged endeavour per urethram. Now come the features to which I wish particularly to direct attention. The needle was driven into the apparently distended bladder without the exit of any urine; it was still more deeply inserted, but with a like result. The needle was then withdrawn and passed in close upon the pubes, and in a direction towards the cavity of the pelvis. Again there was no urine (I need hardly say that we carefully tested our aspirator to see that the fault did not lie with it.) On percussing the abdomen there seemed to be some diminution in the area of dulness, and in examination per rectum there was the feeling of bladder distension behind the prostate, but bimanually no fluctuation could be detected. I confess, in looking back upon the case, this absence of fluctuation or fluid impulse in bimanual examination should have suggested the possibility that we were dealing with a greatly enlarged bladder rather than with a distended one.

Fearing the possibility of any damage as the result of our puncture, as well as the remote possibility of rupture of the bladder, seeing there had been a diminution of distension without any apparent escape of urine, and also, it may be added, for the treatment of the stricture itself, opium was ordered, 1 gr. every four hours, a suppository of camphor and hyocyamus, and the application of hot fomentations.

When seen the next morning he was extremely drowsy, as if under the influence of opium, his pulse and respiration were slow, but his pupils not contracted. He had taken four grains, the fourth grain having been administered but a short time before I saw him. I was sorry for this and regretted that my directions had been carried out rather according to their letter than according to their spirit. Urine was dribbling away by the urethra. In the afternoon I again saw him; he appeared still more deeply under opium, and examination of the abdomen gave the same indication of apparent distension of the bladder. I now tried every possible means to pass a catheter, but failed; and seeing that, although urine was dribbling away there was persistent retention, I deemed it right to operate, and performed Cock's perineal section—that is to say, I opened the membranous part of the urethra at the apex of the prostate.

The next morning the patient, though still in a drowsy semi-comatose condition, could be easily roused, the direct effects of the opium having apparently passed off. As the day wore on he became more deeply comatose, and in the afternoon died. Very little urine had passed through the perineal wound by the catheter.

Had the man recovered, nothing further need have been said about the case; but, as he did not, I am in duty bound to criticise the treatment myself, and I am anxious the members of the Society should do so likewise.

Looking back, then, upon the case in the light of *post-mortem* revelation, and one or two points in the history of the disease which at the time I was ignorant of, I am forced to the following conclusions:—

(1.) That when the patient was admitted he was suffering from uræmic symptoms.

(2.) That the administration of opium, although perfectly justifiable as a combatant against the possible evils—before mentioned—resulting from the treatment, as well as being itself the prescribed treatment for the stricture, would have been much better omitted considering the far graver concomitant of uræmia.

(3.) That the dulness above the pubes was partly due to the distended, but principally to the enlarged, bladder.

(4.) That the needle, when thrust in, either entered an empty bladder or simply passed into the thickened walls.

(5.) That the recession of the bladder, after the unsuccessful attempts at aspiration, was due to the imperceptible escape of urine through the urethra.

(6.) That the patient's death was due to uræmia accelerated, I with regret feel forced to admit, by the administration of opium.

(7.) That the primary inducement of the uræmia was the persistent retention.

(8.) That it would have been wiser to have effectually relieved this distension by an operation earlier.

(9.) That Cock's operation was the best means of treatment, and for the following reasons:—viz., That neither puncture per rectum nor puncture supra pubes could have been with certainty effected. That Syme's operation was impossible. That the ordinary perineal section would have involved a much larger incision and a more tedious operation, both material considerations in such a case as the above; and, in addition, there is the uncertainty of dividing the stricture.

Wheelhouse's operation might have been reasonably considered, but even here, with the numerous false passages and the tortuosity of the stricture, the endeavour to pass the probe might have been fruitless, and thus much valuable time lost, and considerable exposure of the patient involved.

Thus I am reduced to the conclusion that Cock's operation was the best means to adopt; for, while it is simple and rapid in its performance, it is also safe and sure in its results, giving immediate and effectual relief to the bladder and permitting any subsequent treatment.

Without wishing to infer too much from this solitary case, I think it practically supports what we should, without it, theoretically conclude, that, in any case of old standing stricture where there may not unlikely be grave mischief behind the stricture, opium should be given with great caution, if ever administered at all; and also, that when through inability to pass a catheter it is necessary to aspirate, it would be wiser, in preference to aspirating a second time, to at once relieve the tension upon the perhaps already partially disorganised kidneys by one of the operations which I have just discussed.

Dr. M'Ewen asked what Mr. Maylard meant by Cock's operation.

Mr. Maylard, in reply, said that the operation was one chiefly known and practised by men educated at Guy's, and in explanation of the operation read Mr. Cock's account of it from "Guy's Hospital Reports."

Dr. M'Ewen said that he agreed with the conclusions that Mr. Maylard had expressed, but he thought that aspiration of the bladder was a procedure only to be adopted until surgical aid could be procured. Without having known of Cock's operation, he had frequently employed a very similar proceeding in such cases.

Professor M'Call Anderson read the following notes of a case of malignant disease of the pancreas, specimens from which were shown by Dr. Coats:—

Cancerous disease of the head of the pancreas is by no means a common affection; I do not, however, bring the present case under your notice on this account, but because it is illustrative of the difficulty sometimes experienced in forming a correct opinion as to the exact seat of the malignant affection.

This poor woman was 34 years of age, was married, and had had four healthy children; indeed, she had always enjoyed excellent health, and was very active previous to the commencement of her fatal illness.

Her mother is still living, and her only brother died in childhood as the result of an accident; but her father, who was intemperate, succumbed at the age of 60 to some internal complaint, although it is impossible to say whether it was of a malignant nature. About four months before she came under my observation she began to have sick turns, with occasional vomiting; these attacks gradually increased in frequency and in intensity, and about a month after their commencement pain in the epigastrium set in, which steadily increased, and which soon assumed a darting or stabbing character, especially after taking food, or as the result of external pressure. When her stomach was empty she was comparatively easy, but between two and three hours after a meal the pain, accompanied by sickness, set in, which was soon followed by retching and slight vomiting, and for about a month before my seeing her she had on a good many occasions vomited mouthfuls of black clotted blood.

Along with the epigastric pain she suffered from pain in the small of the back; the latter came and went with the former, but differed from it in character, being dull, gnawing, and not so decidedly intermittent.

Her appetite was bad; she feared to take food on account of the distress which it induced, and her bowels had been very costive from the first. She had rapidly lost flesh and strength, and for two months her skin, conjunctivæ, and urine, had been deeply tinged with bile.

The physical examination revealed slight uniform enlargement of the liver, and a tumour bigger than an orange, which was possessed of lateral movement, and had the shape and occupied the position of a distended gall bladder.

She came under my care on the 7th November, and rapidly went down hill with all the symptoms of malignant disease.

There are three points of special interest in this case viewed in the light of the *post-mortem* appearances.

(1.) Gastric symptoms were present from the first and continued a prominent feature throughout, presenting latterly the usual characters of malignant disease at the pyloric orifice, and yet the *post-mortem* examination showed that the stomach was not implicated in the disease. The only way, therefore, that I can account for these symptoms is by presuming that the enlarged head of the pancreas pressed on the duodenum near the pyloric orifice, and that there was some impediment to the emptying of that organ.

(2.) The well marked jaundice, and the greatly distended gall bladder, pointed to obstruction of the common duct, and yet Dr. Coats at the *post-mortem* was able to pass a probe through it. This, however, is not such a remarkable feature, seeing that a slight impediment prevents the escape of bile.

(3.) There was no distension of the superficial veins, no ascites, and no material enlargement of the spleen, no clinical evidence of portal obstruction, and yet the portal vein was involved in the disease, and a probe could barely be passed through it.

These and other appearances will, however, be referred to by Dr. Coats, who made the *post-mortem* examination.

Dr. Joseph Coats showed portions of the liver from this case, and remarked that in the fresh state there were strictly demarcated areas, frequently wedge shaped, which were strongly suggestive of infarctions, such as are commonly seen in the kidney and spleen, but which Dr. Coats had never before seen in the liver. The explanation of this seemed to be that the cancerous tumour in the head of the pancreas had involved both portal vein and hepatic artery, the former where it passes behind the pancreas was greatly narrowed, so that a probe could not be passed. The latter was surrounded by matted tissue, and contained a blood clot, probably a thrombus. Dr. Coats suggested that this double interference with the circulation had probably induced necrosis in certain parts of the liver. The common bile duct was not involved in the tumour, but apparently had been sufficiently pressed on to produce obstruction, which the bile, secreted at a low pressure, was unable to overcome.

Dr. J. Crawford Renton showed a case (which served also to illustrate a neat method of mounting preparations) containing sections of 18 eyes, removed for sympathetic irritation, sympathetic ophthalmia, and severe pain in the remains of diseased eyes.

By an examination of these specimens the great danger of injuries in the ciliary region would be fully established, and it would also demonstrate the long time that a foreign body might remain in the eye without causing any pain or irritation in the eye itself, or the other.

The case of Janet Macpherson, Nos. 17 and 18, was particularly interesting, as showing distinctly ciliary destruction and pressure and necessary irritation of the ciliary nerves.

The globes enucleated were hardened in chromic acid solution, then divided by an ordinary knife and put into a solution of chloral hydrate, which removed the chromic acid. After being 24 hours in this they were put into different strengths of glycerine and water—5 per cent, 30 per cent, and 50 per cent, and then mounted, according to the method recommended by Dr. Priestley Smith, of Birmingham, which consists in embedding the sections in gelatine, placed in glasses made specially for the the purpose in Birmingham. Holding the box with the hooks next the body the glasses are numbered from left to right, No. 1 commencing at the left upper corner.

1. P. C., æt. 33. Traumatic ulcer, staphyloma. Intense pain, enucleation. *Section.*—Lymphy deposit posterior to lens.

2. J. S., æt. 26. Enucleation for sympathetic ophthalmia. *Section.*—General destruction of interior of globe. Lymphy deposit occupies interior.

3. A. W., æt. 27. Injury in ciliary region. Sympathetic irritation. Enucleation. *Section.*—Separation of retina and pressure on ciliary nerves.

4. M. G., æt. 30. Injury with piece of steel, which has penetrated cornea. Enucleation. *Section.*—Foreign body lying behind lens. Destruction of eye.

5 and 6. J. M., æt. 14. Wound of the left cornea. Sympathetic ophthalmia. Enucleation. *Section.*—Destruction and atrophy of iris. Pressure by lymph on ciliary region.

7. W. D., æt. 14. Injury in the ciliary region. Sympathetic

irritation. Enucleation. *Section.*—Large foreign body in the interior of the eye.

8 and 9. H. D., æt. 14. Injury with metal in the ciliary region. Enucleation on account of severe pain. *Section.*—Separation of retina and general destruction of globe.

10. J. P. Injury in ciliary region. Enucleation. *Section.*—Flattening and atrophy of the iris. Retina and choroid thickened.

11. J. F., æt. 20. Injury. Enucleation. *Section.*—Lymph in vitreous and anterior chamber and surrounding lens.

12. M. C., æt. 44. Enucleation of right eye, which was destroyed 20 years ago, but has attacks of pain. Feeble vision in left. *Section.*—Separation of retina and destruction of interior of globe.

13. J. M., æt. 21. Cyclitis, the result of an injury. Lens drawn off. As pain continued excessive, the right eye was enucleated. *Section.*—Atrophy of tissues, which are matted together, which accounts for the pain.

14 and 15. M. J., æt. 23. Enucleated for sympathetic irritation after injury. *Section.*—Atrophy of the lower half of ciliary region. Retina separated.

16. H. D., æt. 24. Enucleation for sympathetic irritation. *Section.*—Lymph around the cilary region.

17 and 18. M. P., æt. 55. Sclerotico-Choroiditis. Enucleation after operation some months ago for anterior staphyloma. *Section.*—Injury to ciliary region, atrophied iris, and lymphy exudation behind retina.

19. James Morris, æt. 42. Enucleation after sympathetic irritation. *Section.*—Adhesive inflammation of all the structures.

20 and 21. J. C., æt. 27. Enucleation on account of pain in the stump of an old injured eye. *Section.*—Foreign body in interior.

22. P. C. Injury in ciliary region. Enucleation. *Section.*—Large solid mass of exudation behind lens, which is shrivelled.

23 and 24. D. D., æt. 26. Wound at upper part of cornea, with prolapsed iris. Vision gone. Complains of intense pain. Enucleation was performed. *Section.*—Lens surrounded by lymph, coloured by blood staining.

Mr. Clark said that the method of preparation seemed to be a good one; but the difficulty he had always experienced in his own trials with such methods had been with regard to air getting access to the specimen. The great objection to mounting preparations in spirit was that it decolorised everything.

Dr. M'Ewen asked what Dr. Renton meant by saying that chloral hydrate removed the chromic acid from the specimens.

Dr. Renton explained that he made the statement on the authority of Priestley Smith, and that the chloral was said to extract the colour from specimens hardened by chromic acid.

Dr. Middleton remarked that a strong solution of sugar removed the colour very well.

Dr. Pollock thought that the chloral might simply act as a preservative, whilst the colour was extracted by the water.

The President remarked that, speaking of gelatine, he had found it useful for protecting the skin of the face, when it was irritable, from the effects of cold winds, &c. The mixture he used was the following:—Gelatin (Coxe's), ℥i; distilled water, ℥ii; carbolic acid, grs. 2 to the ounce. Melt a little of this in an iron spoon, and paint over the tender part. If a little glycerine be applied over the gelatine it will remove any feelings of stiffness or discomfort.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

An Extra-uterine Fœtus in the Abdomen for 56 years.—In the *Comptes Rendus* for 27th August, 1883, M. Sappey gives a very graphic description of an extra-uterine fœtus which had remained in the abdomen of the mother for 56 years. The case is briefly noticed in the *British Medical Journal* for 6th October of this year, under the heading—"A Lithopædion," which means a stone or petrified child, a name quite inapplicable to this case. M. Sappey refers to previously recorded cases, which had remained in the abdomen of the living mother for 26, 28, 30, 31, and 47 years respectively, and then proceeds to describe the present case, which occurred in the practice of M. Beaugendre of Quimpecli, and in which the fœtus was neither petrified nor dessicated. The following graphic description merits a fuller translation:—

"The mother became pregnant at 28 years of age. Arrived at the age of 84, up to which time her health had been fairly good, she was admitted in 1845 to the Quimpecli Hospital, and died three weeks afterwards. M. Beaugendre, who had attended her, made the autopsy. A long incision having been made in the abdominal wall, he was able to determine that the tumour was situated outside the uterus in the course of the right Fallopian tube. This tumour, like all others of the same kind, was composed of a cyst with extremely hard walls, the surface being uneven and tuberculated. The cyst having been removed, they divided it with the aid of a saw into two equal parts. Great was then the surprise of the spectators. In this envelope, which in all its attributes belonged to the mineral world, there was a child! And this child, during its long captivity, had undergone no alteration! It presented itself in the usual attitude, the limbs flexed on the trunk, the head bent upon the thorax. The two completely developed pupillary membranes evidenced that the age of the child was 6 or 7 months. The cutaneous envelope, the superficial organs, the viscera, situated in the great cavities of the body, all the muscles, and all the soft parts, had preserved their consistence, their flexibility, their normal colour. The fœtus in fact appeared, in the eyes of the persons present, as if it were a child just fallen asleep. At this unlooked for spectacle, a thrill of emotion passed through all the assistants, and spread outside with the rapidity of lightning; every one ran to see 'the little old man of 56 years,' as they called him."

M. Sappey ascribes the wonderful preservation of the fœtus to the fact that it had been absolutely preserved from decomposition by the exclusion of putrefactive germs.

Movable Kidney.—In a communication entitled "Einiges über die Wanderniere," in the *Charité Annalen*, 1883, Prof. H. Senator gives an account of 32 cases of movable kidney, which he thinks is more logically designated *Ektopia renis*, and in which the phenomena observed corresponded in almost all points with those recorded in earlier publications, especially the monograph of Landau. According to his statistics one case of renal ektopia occurred in 139 cases of sick women, and with equal frequency in the poor as in the rich. Contrary statements that the affection is less frequent in the wealthier classes, are ill founded or dependent on faulty examination. With regard to the origin of the condition, it must first be stated that absorption of fat is not to be regarded as the cause of movable kidney. That disappearance of the fatty capsule can only have a small influence on the movability of the kidney is seen in the fact that in children, whose capsule contains no fat, renal displacement is seldom or never observed.

Further, in men, who are at least as liable to emaciation as women, the affection is much more seldom met with. Disappearance of fat does not favour the development of the disease, but rather the verification of the diagnosis.

Much more important as exciting causes are the sexual life and clothing of women—(1), repeated pregnancy, and as the result of this relaxation of the abdominal wall (in 42 of Landau's cases 40, and in 27 of Senator's 22 had borne children); and (2) alterations in the position of the sexual organs (Landau), as is easily understood from the close anatomical relationship of these to the kidneys. Further, Landau considers hydronephrosis and tumours of the kidney as causes of displacement, and mentions a case, whilst Senator could not make this observation in any of his cases; and one appears to him to be too little from which to form a conclusion.

Concerning the influence of clothing, tight lacing is to be blamed, a sin of which, as Senator rightly emphasises, women of the lower as well as the upper classes are guilty. Indeed, it furnishes a good reason too, why the right is more often dislocated than the left kidney; the pressure of the corset, &c., can operate directly through the compact and dense tissue of the liver. In this regard, also, the greater length of the right renal artery plays a part in favouring the dislocation of the kidney, as well as, according to Landau, in differences in the disposition of the colon.

The communication terminates with some remarks as to the diagnosis of the condition.—*Deutsche Medizinal-Zeitung*. 30th Aug., 1883, p. 479.—J. L. S.

Air Baths in the Treatment of Fever.—In *Wratsch*, 1883, Nos. 3 and 4, L. R. Fraubenberg, gives results of experiments testing the influence of "air baths," as originally suggested by J. S. Hahn, upon the pulse, respiration, temperature, &c., of patients suffering from fever. The patients were carried into a room of which the temperature could be lowered at will, and laid upon a bed. The skin was then rubbed, either with the naked hand or with a woollen glove, till it was rosy red, and the patient was allowed to lie till the cutaneous hyperæmia subsided or a feeling of cold was complained of, when the rubbing was repeated, and so on during the duration of the bath, which varied from 20 to 60 minutes, but was in most cases about half-an-hour. Taking the average results from the tabulated cases, it was found that temperature in the axilla was reduced by ·74° C. (1·33° F.), in the rectum by ·70 C. (1·26°

F.); respiration became deeper and slower by 6·4 in the minute; the pulse somewhat fuller, and lessened in frequency by 13 beats in the minute.

In the individual cases the results varied considerably, and from these variations the author deduces the following conclusions concerning—

1. The influence of time of day. The greatest fall in temperature was produced between 3 and 9 A.M.; the least between 3 and 9 P.M. These are the normal periods for highest and lowest temperatures, and according to Liebermeister the same condition is observed in the use of cold baths.

2. The influence of the temperature of the room. The baths of lowest temperature 10°-11° C. (50°-51·8° F.) produced least effect; those of 16°-18° C. (60·8°-64·4° F.) much more; while the greatest was produced by those of 13°-14° C. (55·4° F.) In the first case the blood-vessels were caused to contract by the cold, and the temperature rose again; in the second there was not enough heat abstracted from the body. It was noted that the temperature from which most benefit was derived was very different in different cases, and that the temperature of the rooms had to be lowered most in cases where the body was small (being of "less superficial extent"—*körperumfang*), and still well nourished.

3. The influence of the stage of the disease. The later the stage the less the reduction of fever.

4. Duration of the bath. As in cold water baths (Liebermeister) so in these, the effect is not proportionate to the duration of the bath.

5. Conditions of the patient. The younger the patient, the greater the cutaneous surface, and the greater the development of muscular tissue and subcutaneous fat, the greater is the abstraction of heat.

The advantages claimed are that air baths are less objectionable to patients than cold water, especially to children, are more easily managed, and are much less apt to cause over depression. Besides, either over depression or unpleasant feeling of cold can always be prevented by the rubbing.—*Centralbl. f. Chir.* 1883, No. 21.—D. M'P.

Paraldehyde.—Professor Silvestrini shows:—

1. That paraldehyde has a hypnotic action, but less marked than that of chloral.

2. That given in small doses (such as 3 grammes in 100 grammes of honey and water), it produces tranquil sleep, without leaving in the morning a sense of weight in the head,

dulness, or any of the inconveniences generally caused by narcotics.

3. That it augments to a certain degree the urinary secretion.

4. That it does not produce any disorder of the circulation, nor any symptoms of intolerance, and that in fact it is a valuable acquisition to therapeutics.—*La France Médicale.* 18th Oct., 1883.—G. S. M.

Nitrite of Amyl in Pneumonia.—Professor Silvestrini, of Parma, formulates his conclusions as to the value of nitrite of amyl in pneumonia as follows:—

1. In the pre-organic stage of pneumonia, the nitrite of amyl may be of prompt and effective service.

2. One may repeat with impunity the inhalation of this remedy during several successive days, and in doses relatively enormous. (He has administered as much as 50 grammes of the nitrite in five days, the inhalation being carried on for five minutes every half hour.)

3. In cases which have a fatal issue, whether from extension of the pneumonia, or from complications, these inhalations may retard death.

In the brief note of his experience with the nitrite four cases are recorded. In the case of a man, 59 years of age, suffering from double pneumonia which had reached its fifteenth day, the nitrite was commenced under the following conditions: Pulse very small, 120; respirations superficial, 44; temperature, 101·5°; stupor, cyanosis of the face and extremities, coldness of the point of the nose, and tracheal râle indicating the approach of a fatal termination. At the end of five minutes, during which 20 drops of nitrite of amyl were administered, the stupor appeared less, but the other phenomena persisted. The inhalations were repeated during the night, about 10 grammes being thus given. In the morning, pulse 120, respiration 64, temp. 101·3°, with less cyanosis, and signs of commencing resolution. With progressive improvement two days later the pulse was 96, respiration 36, and temp. 100°, and in a few days the patient was convalescent.—*La France Médicale.* 11th Oct., 1883.—G. S. M.

Hydrobromic Acid as a Substitute for the Bromides.—Dr. Dana stated at the annual meeting of the American Neurological Association, that this acid had been used by the profession chiefly with quinine, under the belief that it prevents or lessens cinchonism. The only extended record of clinical observations regarding this acid that he had been able to find was one by Massini, published two years ago, who used it in thirty-one cases of various kinds without special benefit. Dr. Dana was led to experiment with the drug, in the hope that it would produce the beneficial effects of the alkaline bromides in epilepsy without causing depression and scurvy. He had now used hydrobromic acid in the treatment of various nervous affections for nearly two years at the North-Eastern Dispensary, and he had the clinical notes of over fifty cases of various kinds. The officinal dilute acid is a ten per cent solution, of which the dose would be from one drachm to two drachms and a half, well diluted. In *epilepsy* some patients received marked benefit from the use of the acid in doses of four to five drachms a day. Dr. Dana believed, however, that in epilepsy hydrobromic acid could not be used as a substitute for the bromides, except in the non-controllable cases, and yet it undoubtedly has a controlling influence over the disease. In *chorea* he thought the acid could be used advantageously as a medium for arsenic or strychnine when it is desired to give a sedative. In *alcoholism* it failed in two cases, the patients being on the verge of delirium, and the bromides, with chloral, were subsequently given with relief. Hydrobromic acid is a good solvent of quinine, but it *does not prevent cinchonism,* as has been asserted, certainly not in the small doses usually prescribed. In most cases of *insomnia* it also acts well. He could say positively that he could give the acid with just as much confidence that it would produce nervous sedation as when the alkaline bromides are prescribed. He had never seen any sign of bromism, or any disagreeable constitutional effect other than some drowsiness. He believed that the ordinary custom of prescribing from twenty minims to one drachm of the three per cent solution, the strength ordinarily employed, or of a ten per cent solution, was generally much too small a quantity. Theoretically, in order to get the sedative action, from a drachm and a-half to two drachms and a-half of the ten per cent solution must be prescribed. Practically, he had found that very satisfactory sedative effects could be produced with drachm doses of the officinal dilute solution. In conclusion, the acid could be substituted for the bromides in all the milder affections for which the latter are used. It had appeared to him to be especially efficient in producing vascular and nervous sedation in the post and prehemiplegic conditions. Unless given in very large doses, it takes several days to get its best sedative effects. Dr. W. A. Hammond stated that he used hydrobromic acid for seven or eight years, and then abandoned it, because he did not see that it did any good. He had found, however, that it does prevent the unpleasant effects of sulphate of quinine; but in this respect it is not so efficacious as a corresponding dose of the alkaline bromides. Dr. Hammond's experience concerning the power of this acid to prevent cinchonism was corroborated by Dr. Eskridge, of Philadelphia, who also spoke of the good effects of the drug in typhoid fever.—*Canada Lancet.* September, 1883.—J. L. S.

Syphilitic re-infection within 9 years.—Arning records (*Vierteljahrsschr. f. Dermatol. u. Syph.*, 1883, p. 92), the case of a man, aged 34, who was infected with syphilis (chancre and roseola papulosa) 12 years ago. His wife, who does not now show any manifest traces of syphilis, bore after that a boy, still living, and a girl, who died eleven weeks old of intestinal catarrh. The man was infected again through illicit intercourse, and became the subject of a typical primary sore, followed by roseola and defluvium capillorum.—*Centralbl. f. Chir.* 1883. No. 36.—D. M'P.

Salicylate Treatment of Rheumatism.—Dr. Isambard Owen, in an elaborate statistical inquiry into the results obtained in St. George's Hospital in 1877 and 1878 by the use of salicylates in rheumatism, arrives at the following results:—(1) The duration of pain and fever, after the beginning of treatment, would appear to be independent, both of the character of the cases (so far as defined by the range of temperature) and of their previous duration. (2) The duration of the primary attack does not appear to be affected by the amount of the initial doses of salicylate (within the limits given—viz., $1\frac{1}{2}$ to 3 drachms in the 24 hours), or by the combination of full doses of alkali with the drug. (3) The duration of the primary attack, after the commencement of treatment, was only about half as long as under full doses of alkali. (4) In administering the salicylates an advantage is gained in shortening the total duration of the case (*i. e.*, until convalescence is established) by restricting the initial doses and by combining with them full doses of alkali. (5) In respect of actual suffering salicylate treatment showed a marked advantage when compared with alkaline treatment; the advantage was least marked where the salicylate was given in large initial doses without alkali, and hardly more so where the salicylate was given only in small doses; the advantage was more marked for the salicylate in moderate initial doses, and most marked of all where the salicylate was given either in large or moderate initial doses combined with full doses of alkali. In the two latter cases the total duration of suffering averaged hardly more than half that shown in the cases treated by alkali alone. (6) Summing up, the greatest aggregate of advantage is derived from a combination of salicylate and alkaline treatment, the salicylate (of sodium) being given in doses equivalent at the outset to two drachms in the 24 hours, and reduced as occasion requires. The alkaline treatment referred to consisted in the administration of doses of potassæ bicarbonas, or potassæ citras, of sufficient amount to render the urine alkaline. The salicylate was that prepared from carbolic acid.—(*The Treatment of Acute Rheumatism.* Churchill: 1883.) *The Practitioner.* October, 1883.

Two Signs of True Convalescence in Enteric Fever.—In a communication to the Clinical Society of Paris, Dr. Chauffard indicates as sure signs of true convalescence in enteric fever, the occurrence of multiple abscesses and of a critical diuresis. The abscesses have a rapid and insidious development, and when once opened they cease to secrete, their walls uniting in a day or two. The diuresis is sudden, and the quantity of urine passed is very large. Dr. Chauffard's observations extend over the past two years, and in no case has he seen a relapse where these signs presented themselves. —*La France Médicale.* 4th Jan., 1883.—G. S. M.

Subcoracoid Dislocation of the Humerus: Reduction by Kocher's Method.—Prof. Kocher, of Bern, in 1870, published an article recommending a new method for reducing recent subcoracoid dislocations of the humerus; and, in 1881, read a paper before the International Medical Congress, London, on the employment of the same method in luxations of long standing, stating in full the manipulation required, and giving a record of twelve cases from three weeks to four months old reduced by it; in one case in a patient, seventy years of age, he was unsuccessful, fracturing the humerus—the case was of eight weeks' duration.

Reduction, as directed by him, is accomplished as follows: Patient sitting up; the forearm is flexed to a right angle with the arm, the elbow pressed firmly to the side of the chest; the arm rotated outward until firm resistance is met with; then, the arm still rotated, the elbow is carried forward and inward over the chest to near the median line (the elbow should be kept away from the chest during this movement); then the arm rotated inward. The last movement is one of restitution, and carries the hand to the shoulder opposite the one dislocated. These manipulations resolve themselves practically into two movements—outward rotation and flexion. The difficulty experienced in accomplishing reduction of the head of the humerus in subcoracoid dislocation is due principally to the resistance offered to its return by the edges of the rent in the capsular ligament made at the time of the displacement. Traction and the manipulations of the older methods only serve to make the edges of this rent more tense, thus pressing the neck of the humerus more closely against the anterior edge of the glenoid cavity, and grasping more firmly the displaced head. Again, the inferior or posterior part of the capsule and the muscles which pass over and strengthen it cover over a portion of the glenoid cavity, partially occluding it, and in this way offer a further obstacle to the return of the bone to its normal position.

It is claimed by Prof. Kocher that, by the use of the movements recommended by him, the edges of the rent in the capsular ligament are relaxed, more particularly the superior or anterior one formed principally by the coraco-humeral fibres, the obstruction caused by the inferior or posterior one removed, the opening rendered more patent, and the head of the bone approximated to the glenoid cavity. He says: "If now the arm is rotated so that the forearm, bent to a right angle, looks directly outward, this tense band before mentioned, with the head of the humerus, is also turned outward, and thereby the inferior or posterior capsular wall is forcibly lifted away from the glenoid cavity, and the hole in the capsule rendered more patent. But the head of the humerus is still firmly pressed against the anterior edge of the glenoid cavity, because by the motion just made (outward rotation) the upper part of the capsule, as well as its under part, is not in the least relaxed. It is only after flexing, and carrying forward the humerus in its position of outward rotation that the upper part of the capsule is relaxed, and in consequence of the tension of the under part, which still exists and prevents the head from gliding forward, that it (the head) is thrown backward into its cavity," &c.

The *New York Medical Journal* for 29th September contains an account of 21 cases of this form of dislocation treated by Dr. C. A. Jersey. He adopts Prof. Kocher's method, and in seventeen cases the bone was replaced at the first trial without difficulty; in one case some trouble was experienced; in the remaining three cases the reduction was only accomplished after several unsuccessful attempts. Ether was not administered in any case. In a number of the cases the head of the bone was replaced when the elbow was being carried forward and over the chest, a fact to which Mr. Chisholm, of the University College Hospital, calls attention.

In those cases where a repetition of the manœuvres was necessary, it was found that when some traction was made on the humerus in a downward direction, after the elbow had been applied to the side of the chest, and sustained throughout the remaining movements, reduction was effected with little or no difficulty. The advantages claimed for Kocher's method are as follows:—

1st. The control obtained over the humerus by the position of the forearm.

2nd. The advantages obtained by the relaxation of the edges of the rent in the capsular ligament.

3rd. The absence of the necessity for the employment of an anæsthetic.

4th. The absence of pain to the patient, and of discomfort to both surgeon and patient as compared with other methods. —J. A. A.

Histology of the Prurigo Papule.—Although there has been much written about, and many descriptions made of, the histology of the prurigo papule, the opinions of authors have not always agreed; and it was with the idea of settling, as far as possible, the disputed points and differences that Dr. Robert B. Morison, of Baltimore, undertook the following investigations in Prof. Chiari's pathological institute at Prague, on material kindly furnished by Prof. Pick, which was taken *intra vitam* at various stages of the disease under the latter's personal supervision.

He publishes his results in the October number of *The American Journal of the Medical Sciences* for 1883. Dr. Morison draws rather different conclusions regarding the formation of the papules, when considering them in their earliest and latest stages.

He considers that the papule is formed by an infiltration beginning around the upper layer of vessels of the corium, and that this infiltration extending upwards surrounds the papillary vessels, enlarges the papillæ, thus pushing up the epidermis, which becomes thickened at an early stage above them, and at last penetrating it, forms within its layers a small vesicle containing serum, blood, and lymph-cells. The signs of infiltration surrounding the hair sheaths, and sweat ducts are secondary, and they play no especial part in the process. Their presence in the papule is accidental, and it is certain that the primary changes in the skin are not in connection with them.

The colour of the papule at first does not differ from the rest of the surrounding skin, on account of the depth of the slight infiltration with which it begins. For the same reason, it is at first only felt, and not seen, as the infiltration has not extended high enough to push up the epidermis perceptibly, but is sufficiently great to give a feeling of knot-like hardness underneath it. He considers the whole process due to an inflammation, and that all the signs of chronic dermatitis follow regularly, according to the length and duration of the disease, and the amount of scratching, which the itching, as a secondary symptom, causes.

Clinically the formation of the papule coincides with this description, for there is always noticed in the beginning of the disease, after careful investigation of the skin, a slight roughness, and a sensation as of running the hand or finger over small knots, covered with an intervening membrane. At this stage there is no itching. In fact, the itching does not begin until the infiltration has so far advanced that the papules are more distinct. If before this occurs the treatment is begun, no itching appears. This proves, as Kaposi says, that all the symptoms of the disease go hand in hand with the increase or decrease of the papules.

Transfixion of the Brain by a Ramrod—Recovery.— G. Fischer (Hanover) describes a case (*Deut. Zeitschr. f. Chir.*, 1883, Bd. xviii, hft. 5 and 6), in which an iron ramrod, fired from a carbine, struck a servant lad, aged 17, in the back and penetrated his neck and head, so as to project from the forehead. The man staggered when struck, but did not immediately fall, and after a short time became temporarily unconscious. Several peasants tried to draw the ramrod out, but were unsuccessful, though they dragged the lad along the ground by the projecting end of the weapon.

When the man was seen at the hospital, four hours afterwards, about 30 cm. ($11\frac{3}{4}$ in.) of the ramrod was projecting obliquely upwards and outwards from a wound situated 8 cm. ($3\frac{1}{16}$ in.) directly above the left supra-orbital foramen. The scalp, slightly swollen, grasped the metal closely, so that there was no bleeding.

Upon the right side of the neck, near the angle of the jaw, there was a swelling, very tender upon pressure, under which a hard body could be felt distinctly. Behind, at the level of the fourth dorsal vertebral spine, and between the right shoulder blade and the spine, there was found the wound of entrance, about the size of a 10 pfenning piece. The patient could stand upright, was very apathetic, but answered questions correctly.

As the lower end of the ramrod had a thick knob upon it, it was necessary to withdraw from below. Fischer cut down on the swelling in the neck, and found the flattened knob deep beneath the upper end of the sterno-mastoid muscle. After chiselling the vault of the skull to free the wedged-in ramrod, the latter was driven downwards with a hammer, and withdrawn with considerable difficulty from the wound in the neck. There was no bleeding. Sutures were inserted and antiseptic dressings applied.

The ramrod was 50 cm. ($19\frac{3}{4}$ in.) long, and between 6 and 7 mm. ($\frac{1}{4}$ in.) thick; the knob was 4 cm. (about $1\frac{1}{2}$ in.) in circumference. The length of the gunshot wound was about 35 cm. (nearly 14 in.)

From the time of the operation till the twelfth day after, cerebro-spinal fluid flowed from the right nostril, and during the second week there was a discharge of pus from the right ear. The right eye became permanently amaurotic, but there was no other paralysis or disturbance of sense. The frontal wound healed in four weeks. On the 46th day there was discharged from the wound in the neck a twisted piece of cloth 3 cm. ($1\frac{1}{4}$ in.) long, which corresponded with the jacket patient wore when wounded. At the end of nine weeks patient left the hospital and resumed his occupation.—*Centralbl. f. Chir.* 1883, No. 32.—D. M'P.

The Use of Naphthol in the Treatment of Skin Diseases.—Dr. Arthur Van Harlingen, of Philadelphia, reports in *The American Journal of the Medical Sciences* for October, 1883, the results of his experience with the use of this drug, which was first brought to the notice of the profession by Professor Kaposi, of Vienna, about two years ago.

He finds it is one of the most efficient and agreeable remedies for *scabies* which has as yet been brought forward. Both in the rapidity of its action and in its beneficial effects upon the inflamed skin it is superior to any of the means ordinarily employed for the cure of this disease. Its exact place in dermatic therapeutics remains to be ascertained, but he is inclined to think that it will not prove an unimportant one.

In eczema it has failed in his hands to give the same beneficial results as were obtained by Kaposi. In most cases of vesicular and in acute eczema generally its action is simply that of an irritant. On the other hand, it has a limited field of action in the cure of a certain number of cases of squamous eczema of the scalp.

In his opinion it is a valuable addition to our external means of treatment in *psoriasis*. Kaposi speaks well of it in psoriasis of the scalp in particular, and his experience would lead him to place it near chrysarobin and pyrogallic acid in effectiveness without the neutralising disadvantages of either these drugs.

In *seborrhœa* of the scalp naphthol is a decided addition to our means of treatment. While inferior in some respects to sulphur or carbolic acid, it has a certain range of usefulness which further experience will in all probability more exactly demonstrate.

Naphthol is highly lauded by Kaposi in the treatment of *hyperidrosis*, but in Dr. Van Harlingen's hands it has failed entirely, although used strictly according to his formulæ. He considers it quite valueless in this disease, so far as his experience goes.

His experience leads him to regard its effects in *ringworm* as inferior to almost all of the remedies at present used, and as almost entirely inefficient in most cases of *tinea versicolor*.

In *pediculosis* he has had no experience, but in a single case of pediculosis capitis its action was favourable.

Spasmodic Medullary Lathyrism.—This name has been given by M. Proust to an affection met with among the Khabyles of Morocco and Algiers, and characterised by pain in the loins, disorders of motion and sensation in the lower limbs, in the genital organs, and in the bladder. Incontinence of urine comes on suddenly, and there is more or less absolute impotence. The patients required the help of a stick in walking, which they did on the point of their toes, so as even to wear down the toe nails; the tendon reflexes were much exaggerated. The foot was raised with difficulty from the ground, and the effort made to raise it caused a general trembling of the foot. In some cases, M. Proust made out anæsthesia. The pathology of the disease is, according to him, a transverse myelitis, or a hæmorrhage of the cord, with secondary degeneration. The disease he attributes to the poisonous effects of *djilben*, a species of pea analogous to the chick pea (lathyrus cicera), eaten by the Arabs in times of destitution. By some it might be ascribed to cold; but M. Proust thinks that its occurrence always in epidemics, in times of famine, and its never affecting any but the poorest Arabs, who alone are reduced to this diet, indicate the djilben as the cause. The affection has also been observed in animals, and M. Bourlier has induced it in animals by hypodermic injection of an alcoholic extract of djilben.

The treatment consists in the application of revulsives to the vertebral column, and in the internal administration of bromide of potassium. There are a good many cures. In the discussion which followed the reading of M. Proust's paper at the Académie de Médicine, M. Le Roy de Mericourt pointed out the resemblance between this disease and Beri-beri; and M. Bouley reported sundry fatal epidemics among horses and cattle, due to their having been fed on the chick pea.—*La France Médicale.* 5th, 12th, and 19th July, and 9th August, 1883.—G. S. M.

Treatment of Premature Baldness.—Dr. Lassar writes to the following effect on the etiology and treatment of early baldness, or alopecia prematura. From observation and experience upon animals, it was found that the disease is contagious, and occurs independently of any general affection or the state of health of the patient. The method of treatment recommended is as follows: The scalp is to be washed every day with tar soap, or soft glycerine soap, or with soap containing sodium iodide; the soap is to be thoroughly applied, and rubbed into the scalp for fifteen minutes. Following this, a warm douche is used; then after the application of a corrosive sublimate solution (two parts in one thousand)) the hair is dried, and a half per cent spirit solution of naphthalin is rubbed into the affected parts. Carbolic or salicylic acid may also be employed. If this treatment be adopted in the early stage, when the hair is just beginning to fall, it usually proves successful, but it must be kept up for eight weeks or more. The fact that this disease is due to a communicable morbid principle suggests that it may be conveyed by the comb and brush of the barber.—(*Berliner Klin. Wochenschrift.* 16th April, 1883.) *The Practitioner.* September, 1883.

Physometra.—*The American Journal of Obstetrics* for August, 1883, contains an article by Dr. H. C. Yarrow, on the above subject.

In 1872 Dr. Yarrow was hurriedly summoned to see a negro woman, aged about 46, in consultation, and advised to come prepared to perform Cæsarian section, as the woman was four months past her time, and it was feared that the pains, which had begun, would cause rupture of the uterus. The patient asserted positively that she was pregnant, as she had borne children before and was familiar with the symptoms. She said she had felt quickening at the fourth month, and her medical attendant said he had felt the child. On examination the abdomen was found very much distended, laterally as well as in front; breasts full, nipples pouting. A tumour could be felt reaching far above the navel. Percussion, however, was resonant all over, and auscultation revealed none of the sounds to be expected. A uterine sound was passed, the external os being reached with difficulty, and considerable resistance encountered at the internal os, when immediately "there was a rush of pent up gas (quite odourless) in the operator's face, which was most sensibly felt, and which lasted probably not less than half a minute," and the tumour disappeared. "A more careful examination showed that the cervix had been the seat at one time of extensive inflammatory action, and the cicatricial tissue had probably occluded the orifice of the uterus. There was absolutely no information that could be obtained from the patient that would lead to any explanation of her curious condition. She had always been a healthy woman, and remembered no miscarriages, and had never been ill, with the exception of 'womb trouble,' for which she had been treated by her medical attendant.

"A large collection of other cases, considering the rarity of the affection, is made from medical literature. In general, they may be said to represent two forms of the disease—one, like that of the author, which, by an accumulation of gas due to an occlusion of the os, usually from old inflammation, comes to simulate pregnancy, and the other, which discharges the gas from time to time, causing no great dilatation of the uterus, but giving excessive annoyance from the crepitus produced, so that its victim is compelled to forego society altogether.

"Regarding the cause of this singular malady, little seems to be definitely established. The author inclines to the belief that the gas is a morbid *secretion* from the uterine mucous membrane, citing the authority of Dalton, that every organised tissue has the power of absorbing oxygen and exhaling carbonic oxide. In certain cases, when the gas has been fetid, there has been reason to believe that it has been due to the decomposition of retained decidual membranes or other effete products of the uterus. Dr. Charles D. Meigs is cited as believing that is the only way in which any gas can be generated within the uterus, scouting the idea that there can be any secretion of gas by the lining membrane of that organ. This latter authority adds that air may be *drawn into* the womb by suction from without, as that viscus relaxes after expelling clots or other debris in the early part of the puerperium. This is the only condition under which Dr. Meigs admits that any other than a fetid gas can occur in the uterus. Dr. Yarrow admits this piston action of the uterus after labour, but considers that phenomenon quite inadequate to explain such cases as his. His theory of secretion is the one held apparently by a majority of those who express any opinion on the matter."—*Boston Med. and Surg. Journal.* 23rd August. 1883.—D. M'P.

Treatment of Exophthalmic Goitre.—From an experience of upward of seventy cases, and fortified by the unanimous observations of Von Dusch, Eulenburg, Meyer, Erb, and others, Dr. Chvostek is led to regard the rational employment of galvanism as the most important part of the treatment of Basedow's disease. He recommends the following method to be pursued:—(1) the ascending constant current applied to the cervical sympathetic, on each side, for at the most one minute; (2) the same to the spinal cord (the anode at about the fifth dorsal spine, the cathode high up in the cervical region); (3) through the occiput (one pole at each mastoid process), and in certain cases also through the temples, a constant current, for, at the longest one minute, and so weak that the patient can feel but the slightest sensation of burning. Sometimes also local galvanisation of the thyroid gland, with a weak constant current for about four minutes, the current to be reversed at the end of each minute. The applications should be made every day if possible. As a rule, very good results were obtained, even in the most severe cases a cure or marked improvement being recorded. In three cases death resulted from excessive anæmia or complications.—(*Centralbl. für Klin. Med.* 23rd June, 1883.) *The Practitioner.* November, 1883.

Ascarides in the Liver.—Dr. B. Oks reports the following case:—A patient who had suffered for 4 years from phthisis came into hospital because for 2 weeks he had suffered from severe vomiting, while at the same time jaundice had developed and was continuous. The liver was much enlarged. The patient died in a week from exhaustion, and at the *post-mortem*, besides well marked tuberculosis with excavation, the liver was found much enlarged, and in the ductus choledochus was a *female* ascaris, 15 ctm. long, the half of which was free in the duodenum. The bile ducts were greatly distended, and in one of them, in the midst of the liver, was found a *male* worm.

In the literature of the subject many cases have been reported where ascarides have wandered, especially if they endeavour to leave the body shortly before the death of the host; but so far as the author knows, a case has not previously been reported where they have penetrated the ducts of the liver.—*Deutsche Medizinal-Zeitung.* 30th Aug., 1883.—J. L. S.

Treatment of Lupus Erythematosus.—Dr. Fox, of New York, has found the following application useful in four cases of lupus erythematosus:—

Chrysarobin,	15 parts.
Salicylic acid,	10 ,,
Calamine,	5 ,,
Ether,	10 ,,
Flexible collodion,	50 ,,

To be painted on the diseased patches.—*Journal of Cutaneous and Venereal Diseases*, No. 5, Vol. I.

Puerperal Diabetes.—Dr. J. Matthews Duncan has written a valuable paper on this rare affection. In it he gives histories relating to twenty-two pregnancies in fifteen women, varying in age from twenty-one to thirty-eight. So far as is known, all (with one exception) were multiparæ. Of the twenty-two pregnancies in fifteen mothers, four ended fatally after delivery. Hydramnios was frequent, and the liquor amnii in one case contained 0·7 per cent of sugar, and in a second case there was also unquestionably sugar in the liquor amnii, although no quantitative examination was made. In seven of nineteen pregnancies, in fourteen mothers, the child died during the pregnancy, having in all of these reached a viable age. In two more the child was feeble, and died a few hours after birth. In one other case, the child had diabetes. The dead fœtus was very large in many of the cases. Dr. Duncan admits that the number of cases which he has been able to find is too small to possess any very great statistical value; but the histories seem to show that (1) diabetes may come on during pregnancy; (2) diabetes may occur only during pregnancy, being absent at other times; (3) diabetes may cease with the termination of pregnancy, recurring some time afterwards; (4) diabetes may come on soon after parturition; (5) diabetes may not return in a pregnancy occurring after its cure; (6) pregnancy may occur during diabetes; (7) pregnancy and parturition may be apparently unaffected in their healthy progress by diabetes; (8) pregnancy is very liable to be interrupted in its course, and probably always by the death of the fœtus. (*Trans. Obstet. Soc.*, vol. xxiv.)—*The Practitioner.* September, 1883.

Double Ovariotomy (for Carcinoma); Resection of Bladder and Intestine. Recovery.—Dr. Moritz Schustler (*Wien. Med. Wochenschr.*, No. 2, 1883) reports a case from Billroth's clinique, where, during an operation for removal of both ovaries affected with carcinoma, the tumour was found adherent to both bladder and small intestine. A piece of the wall of the bladder, at its superior posterior surface, 3 cm. long and 2 cm. broad, was removed, and the wound closed with silk sutures. A portion of the small intestine, 12 cm. long, had also to be removed. The operation was performed without spray, but under strict antiseptic precautions. Recovery was rapid and uninterrupted. The catheter was used for the first few days only, the bowels responded to an enema on the sixteenth day, and on the twenty-fourth day patient left the hospital well. Subsequent health was good, and fifteen months after the operation there was no sign of recurrence of the disease.—*Centralbl. f. Chir.* 4th Aug. 1883.—D. M'P.

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ORIGINAL ARTICLES.

MALFORMATIONS OF THE HEART IN THEIR RELATION TO THE PATHOLOGY OF CYANOSIS.

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Illustrated by Specimens from Pathological Museum of the Royal Infirmary.

(*Read at the Glasgow University Medico-Chirurgical Society.*)

Gentlemen,—The last time I had the pleasure of being present at a meeting of this Society was in December last, when Dr. Joseph Coats read to you a very valuable and interesting paper on "The Influence of Heredity in Health and Disease." He called your attention to a number of conditions, some of which may be regarded as hereditary; others he described as congenital, while a few of them might be placed in either the one class or the other. To-night I ask your indulgence while I make a few remarks regarding a condition which is generally congenital, but, as far as I am aware, has not been shown to be hereditary, although it belongs to a class of affections which are marked by their tendency to be transmitted in families. The reason why cyanosis has not been demonstrated to be an hereditary condition is probably its comparative infrequency, and the fact that most persons suffering from its graver forms seldom survive to the age of maturity, the malformations associated with it being incompatible with prolonged extra-uterine life. In other cases, however, the cyanotic symptoms do not appear till many years after birth, even although abnormal communications have existed, which admit of free intermixture of the currents of blood between the two sides of the heart.

The term cyanosis has been used by most nosologists as if it were intended to indicate a distinct and definite disease, rather than a symptom of many varying conditions. When the term is used, I hope you will remember that it is applied somewhat in the same way as the words anasarca, albuminuria, or dyspnœa, and is intended to indicate special appearances presented by the patient, which are referable to various derangements of the circulatory and respiratory functions, and not to convey the idea that it is a particular and distinct disease.

Doubtless it is seen most commonly in patients suffering from certain malformations of the heart; but you may also meet with it in other affections, such as effusions into the pericardium or pleuræ, enlargements of the thyroid gland, or aneurisms of the ascending aorta.

The most prominent sign of cyanosis is a discoloration of the tegumentary membranes, particularly the skin, which may assume either a blue or a pale claret colour, varying from a dark leaden or almost black to a discoloration which is hardly perceptible. In most cases, this alteration in colour pervades the entire surface of the body, but, as a general rule, it is most marked on the lips, nose, ears, and extremities. The fingers assume a more or less bulbous form; the feet are apt to become œdematous; and the retardation of the flow of blood through the capillaries is shown by the slowness with which the colour returns to the surface when it has been blanched by pressure. Most frequently the body is emaciated, the abdomen tumid, and the head enlarged. The lungs and heart may perform their functions, so that, under ordinary circumstances, the patient suffers little inconvenience; but there are few examples where, under exceptional conditions—such as active exertion, exposure to cold, or mental excitement—the circulation does not become seriously disturbed.

When the cyanosis has once become established, it may either become permanent or appear only under certain conditions; in either case, there are periods at which the suffering is increased, and the patient is subject to paroxysms of dyspnœa and palpitation of the heart, associated with great irregularity in the force and rhythm of the cardiac impulses, which may terminate in syncope or convulsions. Cyanotic individuals nearly always complain of cold, notwithstanding that the temperature of the body does not differ from the normal.

The frequency and severity of the paroxysms vary greatly in different cases, according to the amount of interference with the circulation and respiration. In the intervals between the paroxysms, the force and frequency of the cardiac impulses appear to be practically normal in some cases, while in others they may be irregular, feeble, and associated with abnormal murmurs or venous pulsations. The duration of the paroxysms ranges from a few minutes to many hours, and they may be of frequent occurrence, or be observed only at rare intervals.

If the cyanosis be not extreme, the patient may live for many years; but in the majority of cases, where the interference with the circulation is considerable, the child does not survive the period of infancy. Death is frequently induced by acute or chronic diseases of the lungs, such as pneumonia, serous effusions, pulmonary atelectasis congenital or acquired, bronchitis, phthisis, and emphysema. Dr. Peacock classifies the causes of death in cases of malformation of the heart as follows:—

"1st. Cerebral disturbance resulting from the defective aëration of the blood, and congestion of the brain.

"2ndly. Imperfect expansion, collapse and engorgement of the lungs.

"3rdly. Effusion into the cellular tissue and serous sacs, from failure of the power of the heart, or recent disease superinduced on the original cardiac defect.

"4thly. Exhaustion from the imperfect performance of the respiratory functions, and the circulation of blood in great part venous.

"5thly. Other diseases predisposed to by the defective conformation of the heart; as apoplexy or paralysis, from engorgement or softening of the brain, or extravasation of blood; epistaxis; congestion or inflammation of the lungs, croup, bronchitis, pneumonia, pulmonary apoplexy, and hæmoptysis, &c.; disorder of the digestive organs, vomiting, diarrhœa, jaundice, &c.; renal affections, &c.

"6thly. Other diseases occurring accidentally, or not necessarily connected with the defective conformation of the heart. Peri-, endo-, and myo-carditis; tuberculous affections of the lungs or other viscera, &c.; embolism of orifices of the heart and of the vessels, especially the pulmonary artery; pleurisy, &c.

"Of these different causes, the two first are by far the most frequent, and especially in children in which the heart is very imperfect, and life is therefore only prolonged for a very limited period. Serious dropsical affections less frequently occur than the degree of obstruction to the circulation would lead us to expect. Gradual exhaustion from imperfect nutrition, and disorder of the digestive organs, are occasionally the causes of death, both in infants and older subjects."

As I stated to you already this evening, cyanosis is not always congenital; it is true that it may not attract attention until some time after the child is born, or its appearance may even be delayed for years; but in a large preponderance of the cases recorded it has been observed to be present at, or very shortly after, birth. When it is not congenital, it may arise from malformations of the heart, which were, at the time of birth, not sufficient to cause serious disturbance. But when the child has grown and the heart has become proportionately enlarged, then the symptoms manifest themselves for the first time. This may be accounted for in two ways, either the cyanosis is caused by a gradual opening of the foramen ovale, by reason of which a mixture of venous with arterial blood is induced, or what is more probable, it is the result of a failure in the development of the orifice or trunk of the pulmonary artery, which leads to systemic stasis in the venous radicles. Besides these causes of cyanosis there are others which must be placed in a different category. I refer to lesions of an inflammatory nature, such as—ulceration, leading to perforation of the auricular or ventricular septa, endocarditis, followed by constriction of the pulmonary orifice, serious pulmonary, or bronchial inflammation, and aneurisms of the aorta.

It is, however, to the pathology of cyanosis arising from malformations of the heart, that I wish more particularly to ask your attention; but before entering upon the purely pathological aspect of the subject it would be well to glance for a moment at the physiological processes by which the development of the heart and great vessels is brought about. I therefore ask you to recall to your minds what you already know regarding this subject, and I will show you how it is that the congenital malformations, which lead to cyanosis, depend upon an arrest or perversion of the normal processes of development.

In its early form the heart is an elongated canal, from the cephalic extremity of which the arterial system arises, while the caudal extremity receives the blood from the venous system. This tube has three dilatations—the bulbus arteriosus, the ventricle, and the venous sinus. From the former of these a number of vascular arches spring, some of which ultimately become obliterated, while others remain, and from them the permanent vessels of the embryo are developed. The heart of the embryo becomes doubly bent upon itself so as to have an S-shaped flexure. The lower part of the S corresponds to the apex, and of the two loops the anterior and upper forms the arterial, while the lower forms the venous portion. At this stage the heart presents the characters of the lower invertebrata. It, however, soon assumes a new form; it is to the changes that I am now about to describe that I wish specially to direct your attention—a series of changes by which the original single auricle, ventricle, and arterial bulb become separated, so as to form the right and left auricles and ventricles, and the aorta and pulmonary artery. A partition gradually arises from the lower part of the right wall and extends from the apex of the heart upwards towards the auricular portion, and ultimately separates the ventricular cavity with two chambers, the right and left ventricles. This separation is completed about the end of the second month. About the same time as this process is perfected a septum between the auricles begins to be formed. It arises from the anterior wall, and grows backwards towards the entrance of the venous sinus, and is not completed during fœtal life, but leaves an opening between the two sides, the foramen ovale, which normally persists till after the birth of the child.

From the arterial bulb the main stems of the aorta and pulmonary artery are developed. It is divided into two parts by a septum which is formed about the same time as the septum ventriculorum, and consists in the projection inwards of two lateral folds which unite and separate the bulb into two distinct channels, the one communicating with the right, and the other with the left ventricle. The septum of the bulb begins at the distal part and advances towards the ventricles, so that it and the septum of the ventricles develop towards one another, and if the conditions are normal the two septa become continuous.

The diagrams now before you illustrate the changes which take place in the development of the heart and great blood-vessels.

Fig. I represents the heart of the embryo at about the fifteenth day, the loop *a* corresponds to the bulbus arteriosus, while *v* represents the venæ omphalo-mesaraicæ, and the two enlargements (*b*) the auricles. Between these two loops there is a central portion (*c*) which is the embryonic ventricle.

Fig. II shows diagrammatically the bulbus arteriosus with five pairs of aortic arches. Numbering from above downwards the central portions of the first and second pairs become obliterated, while of the three remaining pairs the third, with the internal and external portions of the first and second, go to form the internal (*a*) and external (*b*) carotids. The fourth pair on the right side forms the innominate (*c*) and a portion of the right subclavian (*d*); on the left side the arch of the aorta (*e*) and the left subclavian. Of the fifth or lowermost pair the right (*f*) becomes obliterated, while the left (*g*) is developed into the pulmonary artery (*h*) and ductus arteriosus (*i*). These are the changes which take place in the arterial system. I have not said anything about the development of the great veins as they have not such a direct bearing on the subject under discussion; but, before entering upon the pathology of the subject, I should like to ask your attention to diagram III, which represents the course of the fœtal circulation.

The umbilical vein (*a*) on entering the body of the fœtus divides into the following branches:—1st, branches to be distributed to the liver (*b*); 2nd, the ductus venosus (*c*) which enters the inferior vena cava (*d*) after receiving the blood from the portal vein (*e*). The blood enters the right auricle (*f*) by the superior vena cava (*g*), and the inferior vena cava (*d*). The blood from the former passes almost in its entirety through the tricuspid valve into the right ventricle (*i*), while the stream supplied by the latter is directed by the Eustachian valve through the foramen ovale into the left auricle (*k*), and thereafter into the left ventricle. We have now traced the course of the blood into the two ventricular cavities. When the ventricles contract, the left propels the blood into the ascending aorta (*l*), while that contained in the right ventricle becomes divided into two streams—one portion of which is transmitted through the pulmonary artery (*m*) to the lungs, the other, probably the larger, passes through the ductus arteriosus (*o*) into the descending aorta (*n*) and is distributed to the lower extremities.

The changes which occur at birth on the establishment of respiration, are—An increase in the quantity of blood transmitted to the lungs; the ductus arteriosus and ductus venosus become impervious, and the foramen ovale is closed by its valve as soon as the pressure in the left auricle is equal to that of the right.

The development of the heart is now complete. It not unfrequently happens, however, that the changes which I have

FIG. III.

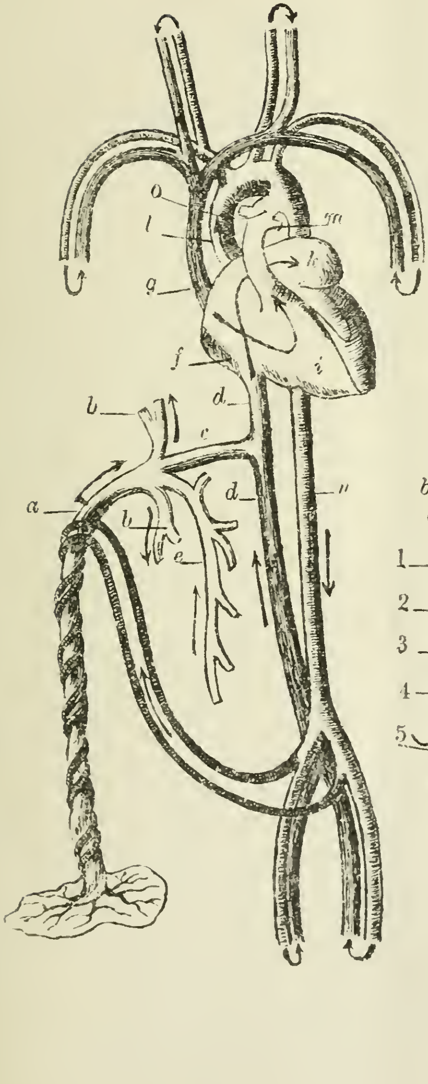


FIG I.

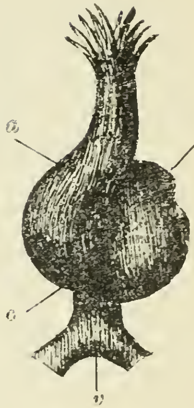
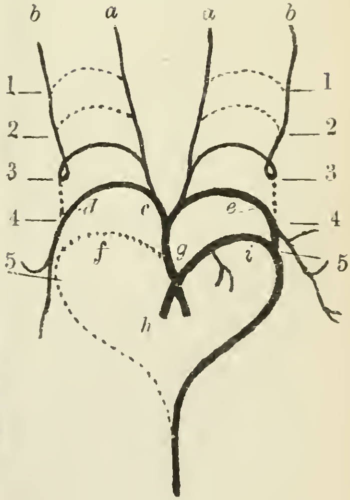


FIG. II.



now so briefly and imperfectly described to you are arrested, and malformations of the heart or great vessels are the result. The character of these malformations depends upon the period of intra-uterine life at which the arrest of development occurs. For example, if the arrest happens about the sixth or eighth week, and should the separation of the left subclavian from the left carotid be retarded, then the two vessels will spring from a common trunk, and you will have a left innominate artery. Or again, should the septum in the bulbus arteriosus not descend so as to unite and form a continuous partition with the septum ventriculorum, then you may have either a transposition of the aorta and pulmonary artery, or both vessels may arise from either the right or left ventricles.

These are examples of malformations occurring from arre t in development during the early life of the embryo; there are others which are normal conditions during fœtal life, but must be regarded as abnormal if they persist after birth; as, for example, permanent patency of the ductus arteriosus or of the foramen ovale.

We have, therefore, by considering the period at which the development of the parts becomes perverted or arrested, a means of classifying malformations. This arrangement has been adopted by Dr. Peacock in his very excellent treatise on malformations of the heart. He says:—

"The cardiac anomalies are classed into—

"1st. Those dependent on arrest of development at an early period of fœtal life, or probably from about the fourth to the sixth week, so that the organ retains its most rudimentary forms—the auricular and ventricular cavities being still single, or presenting very slight indications of division, and the primitive arterial trunk being retained, or the aorta and pulmonary artery very imperfectly evolved.

"2ndly. Those in which the arrest of development occurs at a more advanced period of fœtal existence, or probably from about the sixth to the twelfth week, when the auricular and ventricular partitions have already been partly formed, and the aorta and pulmonary artery more or less completely developed. Such are the cases in which, with imperfect separation of the ventricles, the arterial or auriculo-ventricular apertures are constricted or obliterated, and the origins of the primary vessels, and especially of the aorta, are misplaced.

"3rdly. Cases in which the development of the heart has progressed regularly till the later periods of fœtal life, so that the auricular and ventricular septa are completely formed, and the primary vessels possess their natural connections; but in which, from the occurrence of disease, the organ is either prevented undergoing the changes which should ensue after birth, or there are slighter morbid conditions, which may become the source of serious obstruction at more advanced periods of life. Such are the premature closure of the fœtal passages, and the occurrence of irregularities in the number and form of the valves.

"The irregularities of the primary vessels may be similarly classed into—

"1st. Those taking place at the earlier periods of fœtal life, and consisting in the defective evolution of the aorta and pulmonary artery from the primitive vessel and branchial arches.

"2ndly. Those in which the development of the aorta and pulmonary artery is less deranged, but in which there are defects which may give rise to serious results in after life.

"The former class includes the cases in which the origins of the pulmonary artery and aorta are transposed, or the descending aorta is wholly or in part derived from the pulmonary artery. Of the latter class, the most striking examples are the cases in which the aorta beyond the origin of the left subclavian artery is more or less constricted, and so occasions disease which may ultimately lead to very marked contraction, or even entire obliteration of the canal."*

TABLE I.

Malformations of the Heart.

I. Malformations from arrest of development in the embryo, about the end of the second month.
   a. Heart consisting of two cavities.
   b. Heart consisting of three cavities.
   c. Heart consisting of four cavities, but the septum ventriculorum remaining imperfect.
   d. Obliteration, constriction, or incomplete development of the great vessels. Misplacement of aorta and pulmonary artery.

II. Malformations occurring during the later periods of fœtal life.
   a. Premature closure of fœtal passages.
   b. Irregularities in the valves.
   c. Disproportion of the vessels, orifices, and cavities of the heart.

III. Malformations occurring after birth.
   a. Permanent patency of fœtal passages.
   b. Incomplete development of auricular septum.

* *Malformations of the Human Heart.* By Thomas B. Peacock, M.D. Second edition.

TABLE II.

Lesions of the Heart and Great Vessels which may be accompanied by Cyanosis.

I. Alterations in size or form of cavities.
 a. Dilatation and hypertrophy of right chambers of the heart.
 b. Contracted state of left chambers.

II. Lesions of the orifices.
 a. Obstruction of pulmonary artery from new formations or degenerative changes, or from closure by membranous septa.
 b. Contraction of right auriculo-ventricular opening.

III. Diseases of the great vessels.
 a. Constriction of pulmonary artery.
 b. Dilatation, or aneurisms of the aorta.

Table I contains a list of the malformations of the heart and great vessels which may produce cyanosis, and in Table II, the lesions resulting from diseased processes, which may be accompanied by the same series of symptoms, are enumerated. Let us now consider some of these in detail. For our present purpose the first two divisions, although of considerable interest, are not of great importance, because, in the majority of cases the subjects of them die, if not before, most usually shortly after birth. Some cases, however, have been recorded where children, in which the foramen ovale was widely open, and the septum of the ventricles only rudimentary, have lived for a considerable number of weeks.

The third and fourth divisions should be considered together —namely, the cases where the heart is divided into four cavities, but where the septum ventriculorum remains imperfect. This condition is frequently associated with one or other of the following malformations:—obstruction, constriction, or incomplete development of the great vessels, or transposition of the aorta and pulmonary artery.

The question which now presents itself for our consideration is, why are these malformations associated, and how far do they account for the symptoms which we include in the term cyanosis?

When speaking of the development of the heart, I pointed out to you that the septum in the bulb was formed at the same time as the septum ventriculorum—namely, about the seventh week. The septum of the ventricles arises from the anterior and lower border of the right wall and grows towards the base of the heart, and, under normal circumstances, ultimately becomes united to the base between the two auriculo-ventricular orifices, and with the septum of the arterial bulb.

The last part of the septum ventriculorum which unites with the base is that situated at the root of the aortic bulb, and it is just at this point that abnormal communications between the right and left ventricles are most apt to take place.

The two specimens which I now show you are examples of this malformation (Figs. IV and V).

In both of these cases the imperfection in the septum is found to occupy the undefended space, immediately below the aortic segments, and in neither is there any evidence of obstruction to the flow of blood from the right side of the heart, while there are indications that during life the flow of blood was from the left to the right ventricle. In neither case were there any symptoms during life; it was only after death that the defect in the septum ventriculorum was detected.

The following is a note of the condition of the heart in these cases:—

The first specimen (Fig. IV) is the heart, removed from a patient who died of phthisis pulmonalis in the wards of the Royal Infirmary. In the lungs there were numerous miliary tubercles, and all the appearances of fibroid phthisis, including induration of the apices, emphysema of the anterior margins, dilatation of the bronchi, the formation of small cavities, and the infiltration of the lung, with calcareous nodules. The patient, during life, had no symptoms of cyanosis, and the only abnormal conditions referred to in the ward journal, with reference to the circulatory system, were weak and occasionally irregular action of the heart; apex beat diffused; a thrill felt over the apex beat; and an auricular systolic murmur continued up to and during the first sound. After death the heart was found to be enlarged, and both ventricles dilated and full of blood. A communication (*a*) was found to exist between the ventricles at the base of the heart. The perforation in the septum ventriculorum was situated just below the posterior segment of the aortic valve, on the left side, and after forming a channel of about half an inch in length it emerged at the lower margin of the upper segment of the tricuspid valve. This channel seemed to be lined by a thickened layer of endocardium, which was prolonged beyond the surface of the anterior wall of the right ventricle, and so formed a kind of valve by which blood was prevented from passing from the right to the left side. The tricuspid and mitral orifices, and the pulmonic and aortic valves were healthy, nor was there any evidence of disease or malformation of the pulmonic artery (*c*) or of the aorta (*b*). The patient's age was 36.

FIG. IV.

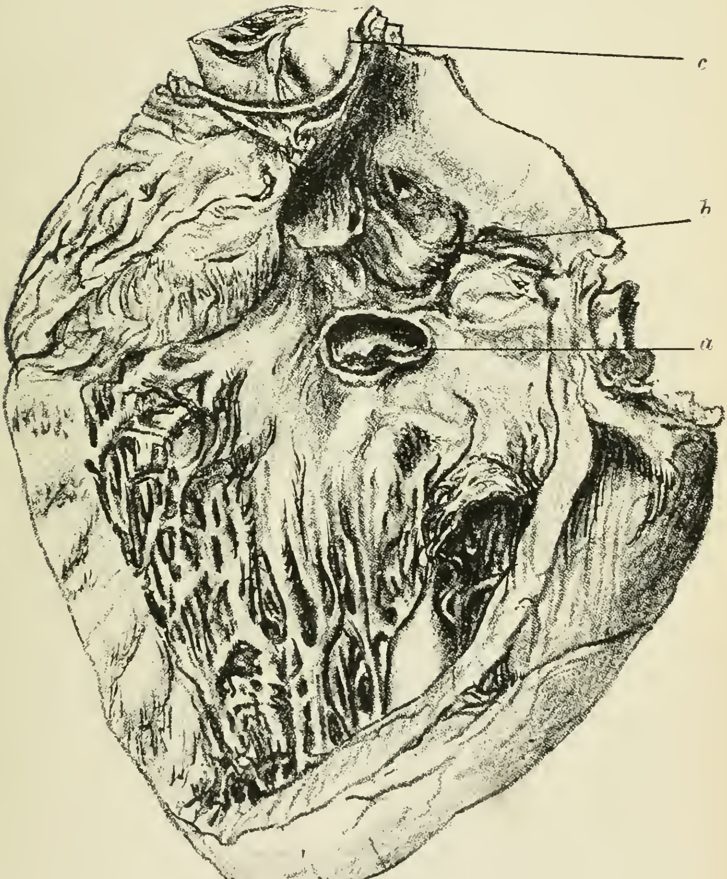
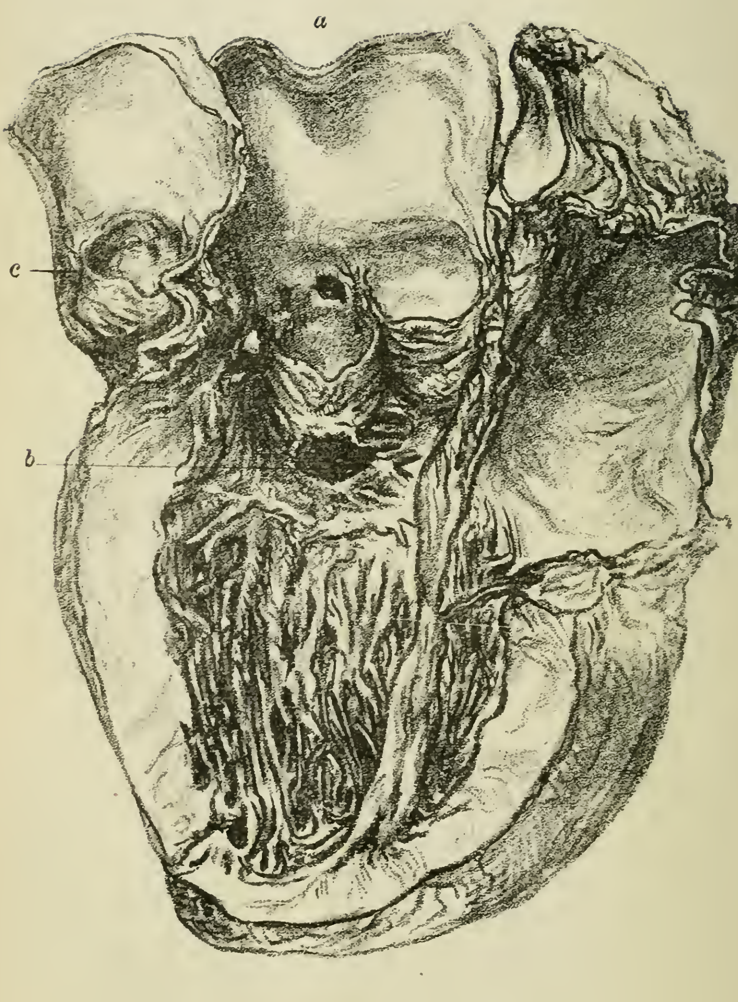


FIG. V.



The second specimen (Fig. V) was removed from a man, aged 34, who, during life, gave no evidence of the condition of the heart found after death, further than symptoms of hypertrophy of the left ventricle and serous effusion into the pericardium. At no period of life had he suffered from cyanosis or other symptoms of a malformed heart. At the *post mortem* examination the pericardium was found to contain about two ounces of serous fluid, and over the anterior surface of the right ventricle, on a level with the tricuspid valve, there was a localised thickening of the pericardium about the size of a shilling. The right and left ventricles were both dilated, and their walls slightly hypertrophied. When cut open the pulmonary artery measured 8·5 centimetres (circumference) and there was no evidence of disease or malformation of its valves. On the anterior wall of the right ventricle a thickening of the endocardium was seen occupying an area of about a square inch, and situated at a point nearly corresponding in position to the thickening of the pericardium above described. Extending between the opening of the perforation in the ventricular septum and this area of thickened endocardium there was a tract of pale thickened endocardium. The left curtain of the aortic valve was considerably enlarged, and the aorta itself, when slit up, measured 9·5 centimetres. The right curtain of the aortic valve was thickened at the line of contact, and immediately below this segment was the opening in the ventricular septum. The margins of its orifice in the left ventricle were rounded and very firm, and were somewhat farther separated from one another than the margins of the opening on the right side. The opening measured transversely 1 cm., and perpendicularly 1·4 cm. The *post mortem* examination, besides showing this condition of the heart, revealed early cirrhosis of the liver with fatty degeneration and pigmentation of the hepatic cells, and also the appearances characteristic of chronic interstitial nephritis.

These specimens, it will be observed, are only deficient in the development of the uppermost part of the ventricular septum; in neither case is there any malformation of the great vessels. Now this is an exception to the general rule; indeed, the association of the two conditions is so common that Dr. Peacock regards obstruction to the flow of blood from the right ventricle by the pulmonary artery, and retarded development of the ventricular septum, as standing in the relation to one another of cause and effect.

Dr. Peacock says:—"When there exists any source of obstruction at the right side of the heart, as in by far the majority of instances in which the septum is imperfect, the course of the blood through the aperture will be from the right into the left; and it is probably only in the comparatively small proportion of cases in which, with a defective septum, there is no source of obstruction on the right side, that the reverse obtains." He accepts the explanation of this condition given by Dr. Hunter. "Dr. Hunter pointed out that if, during the earlier period of fœtal life, when the septum of the ventricles was incomplete, some obstruction arose by which the right ventricle became incapable of freely transmitting the blood which it contained through the pulmonary artery into the ductus arteriosus and descending aorta, the current must necessarily pass through the aperture in the septum into the left ventricle, and thus the final separation of the two cavities would be prevented. To these views it is scarcely possible to object, and it seems equally to result from them that the septum, in its further growth, might be made to deviate to the left, so that the aorta would communicate with the right ventricle."

The theory accepted by Dr. Peacock is a very natural one, but it does not explain the cases where there is no pulmonic obstruction. The cases which I have described do not prove that lesions of the valves, on the right side of the heart, are not the cause, at least in a number of cases, of deficient development of the septum, but they certainly do show that perforation of the septum may exist without any other malformation being associated with it. Obstruction of the pulmonic valve or artery with perforation of the septum do not seem to me in all cases to be related to one another as cause and effect, but rather appear to depend upon a cause common to both. The same may be said of cases in which an incompletely formed ventricular septum is associated with misplacement of the primary vessels. What the causes of malformations are, it is difficult to say, because we have no opportunities of observing them; but I think it may be assumed that they result from some interference with the processes of nutrition, somewhat in the same way as want of growth, atrophy, or degenerative changes, follow upon any impairment of the conditions necessary for healthy nutrition at a later period of life. Now, the area over which this retardation of development extends will depend upon the extent of the interference with the healthy processes. If limited, then the only malformation may be a perforation of the septum, whereas, if more extended, the orifices or valves may be also affected. But what seems to be of more importance than the extent of the interference, is the period at which it takes place. Should, for instance, the development of the septa be checked at the eighth or ninth week, then not only will the partition between the ventricles remain rudimentary, but the aorta and pulmonary artery will arise from a common trunk. Suppose, however, that the development of the septum ventriculorum has become almost completed. During the time that it has been gradually advancing from the apex towards the base of the heart, the cavity of the bulbus arteriosus has also been divided into two channels, the one to become the aorta, the other the pulmonary artery. These two channels may be said to be anterior and posterior to one another only at one part of their course, for instead of the septum in the bulb developing in a straight line, it is formed in a spiral direction, so that the channel which becomes pulmonary artery is in the distant portion of the bulb, situated to the left and posteriorly, whereas at the ventricular end it is connected with the right ventricle in front of the aorta. It is obvious, therefore, that there must be a certain amount of rotation in order that the two septa—the one in the heart, the other in the bulb—may finally unite to form two distinct channels. But should the development of one of the septa be accelerated or retarded, or should the peculiar twisting process necessary for their ultimate union be prevented, then it can easily be understood how the channel which should have united with the right ventricle to form the pulmonary artery, comes to receive the blood from the left ventricle; or, in other words, the two great trunks become transposed.

When the pulmonary artery or orifice is the seat of disease, and has so become incapable of transmitting the necessary amount of blood to the lungs, it may be admitted that the continuous current of blood passing from the right to the left side will prevent its closure. In the same way, pulmonic obstruction may lead to permanent patency of the ductus arteriosus. If the pulmonary artery be constricted, the blood may pass to the left side by two channels, through the foramen ovale to the auricle and through the perforation of the septum ventriculorum to the left ventricle. The blood is then forced into the aorta by contraction of the left ventricle, and, if the ductus arteriosus be still patent, the lung obtains its supply of blood through the same channel. You will therefore observe that obstruction of the pulmonic orifice during embryonic life may lead to three things—(1) incomplete development of the septum ventriculorum; (2) patency of the foramen ovale; and (3) patency of the ductus arteriosus, so that the pulmonic and systemic circulations are maintained by the aorta.

Regarding malformations occurring during the later periods of fœtal life—such as premature closure of the fœtal passages, irregularities in the valve, &c.—I do not intend to make any remarks; but I may show you two specimens in which the semilunar valves are abnormally developed, and then I will ask your attention for a few minutes while I say a word or two about the next class of malformations—namely, those occurring after birth.

In the one case, as the preparation shows, the aortic valve presents only two curtains—a large one on the left, and one of normal size (in the preparation cut through the middle) on the right side. These curtains are generally of normal thickness, there being only here and there, and especially near their insertions, a slight thickening, and even a partial adhesion of their proximal borders; for the most part the curtains are of normal shape, and there is no trace of a third curtain, or of a former division of the existing ones. The malformation does not appear to have led to any inconvenience during life, the patient having presented the symptoms and signs of bronchitis and emphysema. There was considerable hypertrophy of the right ventricle of the heart, but not at all of the left.

In the other specimen, as you will see, the pulmonic valve, instead of having a trifid division, has a small supernumerary segment situated between the right anterior and the posterior segments. The attachments of the extremities of this segment to the inner wall of the artery are fenestrated by marginal perforations of the curtain. The other segments of the valve are fenestrated, but to a less degree, and show also a distinct thinning between the line of contact and the free border.

In these specimens you have examples of malformations of the semilunar valves, due in the first case to an insufficient, and in the second to an excessive, development of the parts.

I wish now to speak of malformations occurring after birth.

Permanent patency of the foramen ovale, or of the ductus arteriosus, is the most important of these. I have already referred to these malformations when they arise from an interference with the flow of blood through the pulmonary artery. There are, however, other cases where the anomaly cannot be connected with any such condition. The auricular septum does not become completed for some time, so that up till the middle of fœtal life there is a free communication between the two auricles; but about that period a fold advances from the posterior wall, and forms a valve, which prevents the blood from returning from the left to the right side, but still allows a free flow in the opposite direction, through the foramen ovale. The foramen ovale may itself be unduly large, the fold just spoken of may not form, or the foramen and valve may be natural, but the latter may not become adherent to the edges of the aperture, as in the specimen I now show you, and so an oblique oval passage may be the result. The last mentioned condition is a very common one, but is not of much consequence, as it seldom leads to any disturbance of the circulation; the others, however, are more serious, and are frequently associated with important defects in the conformation of the heart.

The specimen which I now show you (Fig. VI) is one in which the septum of the auricles is incompletely formed, without any other defect in the development of the heart.

This specimen was taken from a boy aged 9 years, who, having suffered from symptoms of hypertrophy of the heart and valvular disease, associated with albuminuria, died in the Royal Infirmary, 1873; but there were no symptoms, as far as can be made out, which could be described as those of cyanosis. The following is an extract from the catalogue of the Royal Infirmary Museum:—

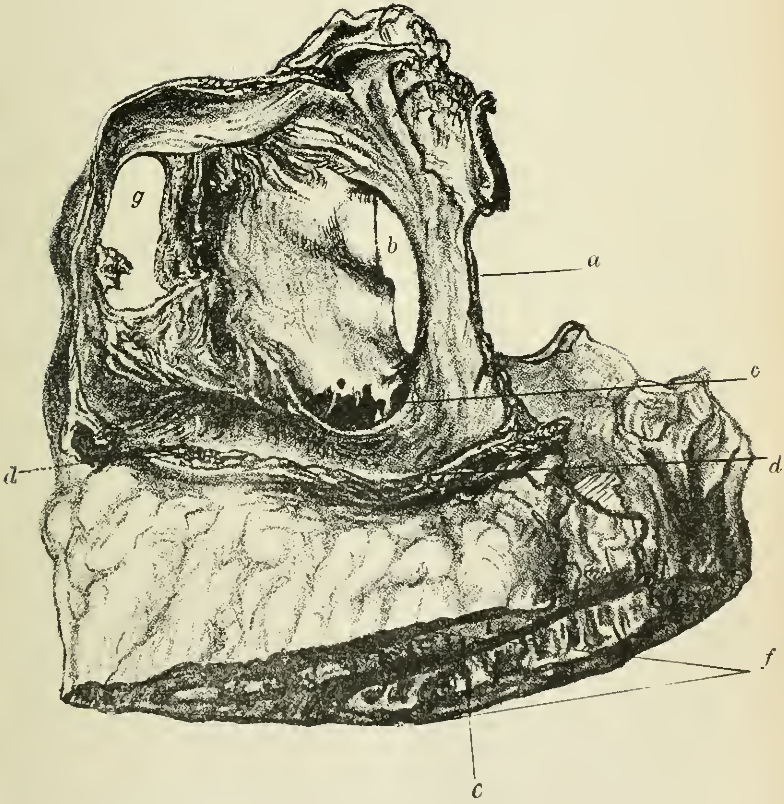
"The aortic valve and its neighbourhood present numerous irregular prominences, almost sufficient to obstruct the orifice. These formations are chiefly attached to the semilunar curtains, but they are also present in considerable mass on the wall of the ventricle beneath the valve, and on the proximate curtain of the mitral. In fact, the vegetations in these latter positions form a kind of second aperture half-an-inch beneath the level of the aortic valve, and into this aperture project the more bulky vegetations of the curtains, in such a manner as almost to obstruct it. The aortic valve presents only two semilunar curtains of nearly equal size. These may be distinguished as a left posterior and right anterior; the former being nearly normal in shape, but large; the latter showing an indication of division into two, as follows:—from a point about half-an-inch above the middle of the valve a narrow band passes down the aortic wall, to which it is firmly adherent: at the base of the curtain this band divides into three smaller ones, which spread out on the semilunar fold. The appearance reminds one to a certain extent of a columna carnea, giving off its chordæ tendineæ. In the semilunar curtains there is no appearance suggestive of chronic endocarditis, the curtains being comparatively thin, except where the vegetations are attached. Just above the valve there is a pouch in the aorta, projecting to the right. Viewed externally this pouch is about the size of a half walnut. Internally it presents numerous irregular vegetations. The foramen ovale is found to be pervious, admitting a tube of a quarter inch diameter; the aperture is widest on the right side and passes obliquely forwards to the left, so that on the left side it is almost valved. The heart was very much enlarged, weighing 11½ oz. (in a boy of nine years). It extended from the right border of the sternum as far as two inches to the left of the nipple. The spleen was enlarged (8½ oz.), and it, as well as the kidneys, presented old and recent evidences of embolic phenomena. Patient presented, during life, symptoms of cardiac disease with albuminuria. There was a loud murmur and fremitus v.s. near second right costal cartilage."

I here show another example of patency of the foramen ovale. The channel of communication, in this specimen, between the right and left auricles is about a quarter of an inch in length and sufficiently large to admit a No. 9 catheter. The pulmonic orifice is small in size and the segments of the valve are somewhat contracted. The aortic valve is normal. In this case there was a history of cyanosis, but no details regarding the case can be procured.

In the next specimen you will observe that the foramen ovale is open, but is included in the edge of the Eustachian valve, and so a current of blood is prevented from passing from the right to the left side. In this case also there were no symptoms of cyanosis, even though the patient suffered from extensive emphysema and congestion of the lungs, and ultimately died from pneumonia and basilar meningitis, associated with tubercular mischief both in the brain and in the lungs.

These are three examples of patent foramen ovale. I will now show you a specimen in which the auricular septum is free at its anterior margin (Fig. VI). You see by looking at the specimen that the walls of both auricles are extremely thin, the septum auriculorum is incompletely developed at its anterior margin, and at its lowermost part there is an opening divided into three parts by slender bands of fibrous tissue, which give it a fenestrated appearance. The opening at the anterior margin is about ¾ inch in length, and when the septum is on the stretch, about ¼ inch in breadth. The septum appears to be composed entirely of fibrous tissue, there being only a few bands of muscular fibres visible. The right and left auricles are about the same size, but the walls of the left are slightly thicker than those of the right. The ventricular walls are

FIG. VI.



normal in thickness, and the cavities are slightly dilated; all the valves are strictly normal.

The malformations I have been speaking of are entirely wanting in pathognomonic symptoms. Certainly the graver forms—such as occlusion of the pulmonary artery with incomplete development of one or both septa of the heart—may cause death either before birth or within a limited time after it. Although early death is the general rule in such cases, in a certain number an amount of compensation is established, which, though inadequate to protect the patient from considerable inconvenience, is sufficient to maintain life. Blueness of the skin (cyanosis) is the most important, and was at one time considered to be a pathognomonic sign of congenital malformation of the heart. It is now known, however, that it is only in the graver cases, where there is only slight compensation established, and where, therefore, serious disturbance to the circulation exists, that this symptom is observed in early life. But, even though it may be absent for some time after birth, a period in life usually arrives when the compensation becomes insufficient, and cyanosis is then for the first time observed to be present.

I have shown you two specimens of perforation of the septum ventriculorum, three of open foramen ovale, and one in which there was a free communication between the auricles. Now in only one of these was there a history of cyanosis.

I have already classified malformations according to the period of embryonic life at which the arrest of development occurs. The following arrangement is based upon the effects produced by the various malformations, and for this purpose they may be divided into four classes:—

I. *Conditions which lead to a deficient flow of blood to the lungs,* such as constriction of the pulmonary artery or orifice, or of the tricuspid valve, &c.

II. *Conditions which permit of communication between the venous and arterial circulation,* such as patent foramen ovale, perforation of the septum ventriculorum, patent ductus arteriosus, and misplacement of the primary vessels, &c.

III. *Conditions which cause venous blood to be distributed to the systemic arteries and arterial to the pulmonic vessels without any considerable mixture of the currents,* as, for example, cases of transposition of the aorta and pulmonary artery, with the venæ cavæ normal. In such cases the only possible communications between the two sides of the heart are—by a patent foramen ovale, an open ductus arteriosus, or a perforation in the septum ventriculorum.

IV. *Conditions which retard the return of blood from the lungs to the left side of the heart*, such as narrowing of the pulmonary vein, stenosis of the mitral orifice, and contraction of the left chambers of the heart, &c.

Now these congenital malformations may lead to cyanosis in early life, or the patient may not suffer much inconvenience until much later, when, by a continual decrease in compensation, or from the development of organic disease of the heart, the circulation is so much interfered with that he becomes cyanotic. What, then, are the theories by which cyanosis is explained? There are two advanced.

(1) The venous stasis theory, and (2) the theory that cyanosis is caused by an intermixture of venous and arterial blood.

Let us first consider the intermixture theory. In one of the specimens I showed you this evening you observed a large opening in the auricular septum, through which a free intermixture of venous and arterial blood was possible, and yet there was no evidence of cyanosis during life. Again, I showed you two hearts, with perforations in the septum ventriculorum, unassociated with any other malformation, and in these cases also there was no cyanosis. Symptoms may also be wanting when the incompletely developed ventricular septum is accompanied by constriction of the pulmonary artery, or even where the aorta arises wholly from that vessel.

The theory, therefore, that cyanosis is caused by a mixing of venous with arterial blood cannot be sustained, although it has long been received as an explanation of the symptom we are now considering. In the fœtus you have a mixture of venous and arterial blood, and yet there is no evidence that it is cyanotic during intra-uterine life. It is true that infants at birth are often of a dark blue colour, but the colouration is due not to the mixture of venous and arterial blood, but rather to obstruction to the circulation during delivery. The cyanosis in such cases passes off rapidly as soon as the obstruction is relieved and respiration established, even though the fœtal passages remain open for some time. And further, it has been clearly shown that cyanosis may exist without any mixture of the currents of blood; and, in those cases where cyanosis is present, the intensity of the colouration has no relationship to the amount of venous blood in the arterial portion of the systemic circulation. Dr. Hale records a case where there was only one ventricle, the foramen ovale was widely open, and the aorta and pulmonary artery arose from their normal position, in which the surface of the skin was normal in appearance.

The venous stasis theory also proves inadequate to explain all the cases of cyanosis; it must be admitted, however, to be the more plausible theory of the two. Free and uninterrupted circulation of the blood through the capillaries is necessary to the maintenance of the normal colour of that fluid. Should partial stasis be produced, even though the blood flowing to the part be entirely arterial, the surface will exhibit a more or less purple tinge, the depth of colour being in an inverse ratio to the rapidity of the circulation; the longer the blood remains in the capillaries, the darker will it become. Deepening of the colour of the integuments is seen towards the close of fevers terminating fatally. In such cases a cyanosis is produced by stagnation of the capillary circulation, as a result of feeble cardiac action from fatty degeneration and atrophy of the heart.

The observations of Dr. Stillé go to show that obstruction of the pulmonary artery is the most important cause of cyanosis; but contraction of the tricuspid orifice, or atrophy of the walls of the right ventricle, also lead to serious symptoms and marked discolouration of the skin.

The argument brought against this theory is,—that there is no relationship between the intensity of the cyanosis and the amount of contraction of the pulmonary artery. It must, however, be remembered that in a large number of cases there are means by which pulmonic obstruction may be compensated for. For example, in a case where there is obstruction of the pulmonary artery, associated with patency of the foramen ovale and ductus arteriosus, the blood from the right side of the heart may escape in two ways—(1) through the pulmonary artery (the quantity being determined by the amount of obstruction), and (2) from the right to the left auricle through the foramen ovale, where it unites with the blood from pulmonary vein, and thence through the mitral opening to the left ventricle. On contraction of the left ventricle, these united currents are propelled into the aorta, and for the most part enter the systemic circulation, but a portion of the blood may be sent to the lungs, (1) by the patent ductus arteriosus, (2) by a collateral circulation through enlarged bronchial arteries, or branches of the subclavian or coronary arteries; therefore the quantity of blood entering the left auricle from the pulmonary vein may be considerably in excess of what is sent to the lungs through the obstructed pulmonary artery. Without the existence of some abnormal channel through which the blood may pass, life, in such cases, could not be maintained for the shortest time after birth. The obstruction to the pulmonic opening is therefore no guide to the amount of blood passing to and from the lungs. In cases where the pulmonary artery is obstructed, and there is no opening either in the auricular or ventricular septa, the probability is that the pulmonic circulation is carried on to a great extent through the collateral circulation I have just described. There is little doubt that in such cases, should the compensation be sufficient to prevent the development of cyanosis until late in life, that by that time the capillary vessels will be in a more favourable position to offer resistance to the pressure from within than the more pliant capillaries of the fœtus or new born infant. The cases of cyanosis, other than those associated with malformations of the heart, also support the theory that obstruction on the right side of the heart has a great deal to do with the pathology of the subject. It is in cases of dilatation and hypertrophy of the right ventricle, associated with tricuspid regurgitation, obstruction of the pulmonary artery from new formations or degenerative changes, constriction of the tricuspid valve, atrophy of the lungs, and aneurisms of the ascending aorta, that cyanosis is met with. But cyanosis may also be the result of deficient propulsive power on the left side of the heart. Should the left ventricle, from weakness in its contractions, be unable to overcome the normal resistance to the flow of blood in the systemic circulation, it is evident that very much the same condition of stagnation of blood in the capillaries will result, as when there is an increased resistance to the return of blood to the right chambers—namely, the rapidity of the capillary circulation is reduced, and therefore the dark colour of the blood is increased. But whereas in cases of tricuspid regurgitation and dilatation of the right ventricle there is a tendency to dilatation of the venous radicles, in obstruction to the flow of blood from the left cavities there is stasis without increased pressure, therefore without this tendency to dilatation of the veins or capillaries.

To sum up therefore,—It appears that the most important cause of cyanosis is obstruction to the flow of blood to or from the right side of the heart, and that the degree of the discolouration depends upon—

1. The time at which the obstruction became sufficient to give rise to cyanosis. The earlier the symptoms appear the more marked are they likely to be.

2. Free communication of venous and arterial blood in so far as it determines the resistance to the flow of blood to or from the right chambers of the heart, and the amount of blood carried to the lungs.

3. The amount of compensation established by a patent

foramen ovale, or ductus arteriosus, or by a collateral pulmonic circulation.

4. The nature of the vessels and integuments—the thinner the vessels and other textures the deeper the hue.

5. The degree of hypertrophy of the right ventricle and the condition of the tricuspid valves.

In these remarks I have given you a short, and, I am afraid, very imperfect sketch of the subject; to treat it at all in a thorough way would require a much longer time than I can ask you to place at my disposal; but, if I have succeeded even in giving you some idea of the malformations to which the heart is liable, and the connection between them and the discolouration of the surface which forms so marked a feature in most instances, then I do not think our time has been misused. I hope I have also demonstrated to you, particularly to the junior members of the Society, the importance of the study of embryology as a means of preparation for the proper understanding of morbid anatomy. Without a knowledge of it congenital malformations of the heart would only appear as mystical abortions without any scientific explanation.

DESCRIPTION OF LITHOGRAPHS.

Fig. I represents the heart of the embryo at about the fifteenth day, the loop *a* corresponds to the bulbus arteriosus, while *v* represents the venæ omphalo-mesaraicæ, and the enlargement (*b*) the auricle. Between these two loops there is a central portion (*c*) which is the embryonic ventricle.

Fig. II shows diagrammatically the bulbus arteriosus with five pairs of aortic arches. *a*, internal, and *b*, external carotids, *c*, innominate, and *d*, right subclavian arteries, *e*, aorta, *f*, obliterated fifth right arch, *g*, fifth left arch, *h*, pulmonary artery, *i*, ductus arteriosus.

Fig. III. *a*, umbilical vein, *b*, branches to the liver, *c*, ductus venosus, *d*, inferior vena cava, *e*, portal vein, *f*, right auricle, *g*, superior vena cava, *i*, right ventricle, *k*, left auricle, *l*, ascending aorta, *m*, pulmonary artery, *n*, descending aorta, *o*, ductus arteriosus.

Fig. IV. *a*, perforation in septum ventriculorum lined by a thickened layer of endocardium, *b*, the aortic valves, and *c*, the pulmonary artery.

The heart is viewed from the left side, the anterior and a portion of the left wall of the ventricle having been removed.

The drawings of Figs. IV, V, and VI are full size, but the specimens, having been in spirits for several months, have become somewhat reduced in size.

Fig. V. *a*, aorta, *b*, perforation in septum ventriculorum, viewed from left side, *c*, pulmonary artery.

Fig. VI. *a*, anterior wall of right auricle, *b*, anterior opening in septum auriculorum, *c*, perforations at lower part of septum auriculorum, *d*, floor of right auricle, *e*, septum ventriculorum, *f*, walls of right and left ventricles, *g*, opening of superior vena cava.

SCHOOL WORK IN ITS RELATION TO HEALTH.

By JOHN STRACHAN, M.D., Dollar.

For some time past the country has been congratulating itself on the great success which has attended the working of the recent Education Acts; and, on first sight, the facts annually presented to the public by the Education Department would seem to indicate that there is good ground for so doing. The enormous strides which have been made in "schooling" during the last ten years seem to show both the need there was for the Acts in question, and how well adapted they have been to effect the purpose required. School accommodation, teaching power, and the attendance of pupils, have all been more than doubled since the passing of the Act, in 1872; and what with School Boards, compulsory officers, and payment by results, the whole scholastic machinery is worked with a vigour and thoroughness from which no child can escape; while bursaries and scholarships innumerable, competitive examinations and "locals," are giving an impetus to learning which bids fair, so far as young people are concerned, to cast all other considerations and requirements into the shade.

Could we look upon schooling as an unmixed good, unattended by any counteracting evil, such a state of matters would be extremely gratifying, and we should look forward with confidence to a greatly improved condition of society, when the full effect came to tell upon the adult population.

The satisfaction we might otherwise feel, however, is considerably marred when we regard the matter from the point of view of the medical practitioner, and recognise in the school system which is thus being extended and stimulated an old *causa morbi* with which we were only too familiar in the past, and which promises to yield fruit more and more abundantly under the forcing to which it is now subjected. When we reflect upon the close relation which has always subsisted between school life and certain forms of illness among the young, and more especially the frequency with which diligence and success at school have been associated with delicacy of constitution, nervous disorders, brain affections, and defective development, we may well, like Sir Lyon Playfair, "look with jealousy and suspicion on the rapid growth of examining systems through competitions and examinations," and on the "changes in our school system intensifying the evil." *

* Speech in the House of Commons, 26th July, 1883.

Without wishing for a moment to detract from the benefits which undoubtedly flow from extended school teaching, as applied to many who might otherwise have remained in ignorance of even the elements of education, and from stimulating others to carry their studies into higher regions than they did formerly, it is still a question whether these may not be bought at too high a price, if, at the same time, the evils which have, as undoubtedly, attended school life, be aggravated and extended. As a profession, it is with the latter consideration that we have to do. It is our duty to try to free school teaching from these dangers—to keep it safely within the bounds of physiological and sanitary conditions; and it is we who are properly responsible for errors in this respect.

Dr. Forster, of Birmingham, in his presidential address last year to the Midland Counties' Branch of the British Medical Association, dwelt eloquently and forcibly on the failure of the medical profession to exert its due influence on the legislative councils of the nation. This failure, I venture to assert, is nowhere more conspicuous than in matters relating to the education of the young; and nowhere, I believe, could the want of that influence have more serious consequences. Probably every medical practitioner in the kingdom has had frequent occasion, in the interests of health, to step between the school and the pupil, and can point to many instances in which work, at and for school, has been the cause of serious, and even fatal illnesses in the young. Yet this very serious feature in the past history of school life is being increased rather than lessened by the great educational activity at present stirring in the country, and has been altogether ignored in framing the Education Acts. The medical profession, who alone are qualified to raise an authoritative voice in the matter, have, as a body, been silent, and no warning has been given of the dangers to be avoided. The authorities may, therefore, be excused for acting as though no danger existed—pushing on the existing procedure as though nothing but good could result. Those, however, who have had experience of the effects of excessive school work, may well look with grave apprehension upon the strenuous efforts, legislative and voluntary, everywhere being made to extend and intensify the motives for such work. It is as though premiums for speed were offered to engine drivers upon a railway system—which had been proved by numerous accidents to be very defective and unsafe—without using any means to remedy the defects, or to limit the speed upon unsafe parts of the line. We might imagine keen business men—whose only thought was for expeditious travelling, unaware of the accidents which had occurred, and taking it for granted that the lines were all right—making such a mistake, *if the engineers* kept silence as to what they knew of the real state of matters. Medical men, being the professional exponents of physiological law, hold, or ought to hold, the position of engineers of the educational system; and it is they, and they only, who can say when the lines pursued are safe and trustworthy. It is for them, therefore, to speak out and give effective warning of obvious dangers in school life, arising out of a departure from physiological principles. Had this been done by the profession at large through its representative bodies, when the Education Act was under consideration, important modifications would surely have been effected, and much subsequent evil averted. But there is no indication in the framing of the Act—dealing with the most important and most delicate of all vital processes, the growth of the human brain and the development of mind—that medical opinion was ever consulted, or had the least influence, unless it might be in the comparatively small matter of ventilation and air space.

The Act is entirely one-sided, in the sense that it aims simply at urging on school work, and takes no account of the dangers which may attend the process. It insists upon all children, not otherwise educated, being sent to school; but it does nothing to protect them from injury when there. It exacts a certain minimum of school attendance per day, but it does not limit the time during which a child may be kept in school or at school work. It prescribes certain standards of educational attainment through which the children have to be passed progressively, and bribes the teacher to "pass" as many as he possibly can; but it imposes no restraint as to the method of teaching to be employed or the treatment to which the pupils may be subjected in the effort to "work them up" to the required standard. It offers extra payment to the teacher for passing his older pupils in certain special subjects; but takes no care to ensure that these be not imposed as an extra and injurious burden upon the child.

It is perfectly well known and admitted by the highest authorities, that the methods employed in many "Board Schools" are very defective and unsafe; yet the pressure of the Code, and the whole machinery of the Education Act, are applied to all alike. It may be quite true, as claimed by Sir Lyon Playfair, that the requirements of the Code are not excessive—are only such as, with *proper methods,* may be readily attained without over pressure. But the question is, are they so attained? or does the Act, in its working, tend to produce over pressure? Is it safe to urge the train of educational attainments to increased speed upon the lines as at present existing in the country? From all parts of the country we hear complaints, by teachers, as to the amount of pressure they find it necessary to put upon some of their pupils, in order to keep up their percentage of passes. Mr. Davis, School Board Clerk, Birmingham, puts the position of affairs very clearly, in a paper read to the School Board Clerks' Conference, held at Swansea last year. After showing very clearly how much it is the teacher's interest to have as large a percentage of "passes" as possible, he says:—"However fond he (the teacher) may be of children naturally, he learns to look upon his scholars as so many little machines, out of whom he has to grind certain results. The average children do not bother him; steady work secures their passing. But the weaker children must receive concentrated attention. What they cannot accomplish in the ordinary school hours, they must be driven to in extra hours; and if they flag in their efforts, they are likely to be spurred on by punishments of some kind or other." And again—"Those (children) who have to be presented on very few attendances are certain to have to bear an amount of pressure which is altogether excessive." Without commenting upon the morality of the spirit in which teachers are thus represented as dealing with the children entrusted to their care, except to say that it seems something akin to that of baby farming, I think the above, and very much more of the same kind which might be quoted, may be taken as evidence of the very one-sided working of the Education Acts, and of the great need there is of medical supervision, if not of legislative restriction of the burden of school work which children are called upon to bear.

It is not necessary to suppose teachers selfishly regardless of the health and future welfare of their pupils, although ready to inflict suffering for the declared purpose of advancing their own interests. Being, like the rest of the community, ignorant of the laws of health and of development, and having, perhaps, an exaggerated opinion of the value of school work, their thoughts and actions are very apt to go entirely in the direction of the latter, unchecked by due consideration of the former. Even when grinding results out of their pupils, and applying pressure which they admit to be altogether excessive, they are, I believe, honestly of opinion that it is all for the good of the child. There is, indeed, no new departure as to the amount of work sought to be imposed upon the child, that having always been, as now, pretty much all that he could be got to do. The effect of the Act is not to make the teachers give more work, but to cause them to insist upon that work being done by the whole which formerly was only done by a few at the top of the class. Before the passing of the Act, teachers were content to depend chiefly on the stimulus of emulation and ambition to lead on the few to strain their powers over their books; now they are impelled by self-interest to spur on the remainder to do the same amount of work. Formerly, only the keen, ambitious prize takers, suffered in health, because they only were induced to overwork themselves; now, in Board Schools, the dull and slow pupils run the greater risk of injury from the means used to work them up to the required standard. But the teacher, I believe, has no more idea now than he had formerly that he may be doing actual or permanent injury to the child. He may pity him for his pale cheeks and aching head, much as he would for his smarting palms after the application of the tawse; but he has a vague conviction—sadly at variance with the results of past experience—that the man will be all the better for the suffering and hard work of the boy.

It may well form a question for serious consideration, whether those entrusted with the management of the most important and most difficult to understand of all physiological processes, should not themselves possess a thorough practical acquaintance with the laws and principles of physiology, upon which lines alone the process can be conducted with safety. But seeing that at present this is not so, it is incumbent upon the only class of men who possess that knowledge to speak out and show plainly what are the lines of health upon which education should proceed.

In order to form a full estimate of the dangers attending school life, it is necessary to keep before us the normal course of development upon which ultimate strength and health so greatly depend.

The period of youth is that devoted by Nature to preparation for the coming life; and it is to this end that the characteristic features of children, as of other young animals, are designed. From the time of birth, a gradual but steady process of growth and development of the whole organism goes on continuously, day by day, till it culminates in the adult animal. By the added increment of each day's growth, the entire organism is built up; and since the period of growth is limited, the addition of each day bears a definite relation to ultimate size and strength. As in other animals, and in plants, this process, from beginning to end, and in all its parts, is spontaneous in its action, by virtue of an inherent vital property, which may be greatly aided from without, as in the cultivation of plants, by rendering surrounding conditions favourable, but cannot be forced or controlled, even to the extent of making one hair grow long or short, white or black.

This process of spontaneous development, forming our whole nature—body and brain, mind and morals—just as it forms the whole nature of every plant and animal in the universe, constitutes the true scientific basis of education. It builds up the mechanism through which alone educational attainments can be received and applied, and upon the strength of which the power of mental and bodily activity must entirely depend. It is by guiding, aiding, and supplying this process that we can truly cultivate the nature of man, and bring his physical, moral, and intellectual powers to their full and healthy development; while anything interfering with it must curtail, to the same extent, the powers of life and action.

The first requisite for full development is sound and vigorous health. Any departure from this condition carries with it a like diminution of developmental activity, and, consequently, of ultimate strength. The defective growth of ill health may be seen in the case of the nails. The part which grows during an illness is thin and weak, and may thus be distinguished from the healthy growth by a depression, which may be of importance in a medico-legal sense, as fixing the date of the illness. A like defect in the growth of the hair causes it to break off, or, in popular phrase, "come out" after an illness. The nails and the hair go on growing indefinitely, and the weak parts are, in course of time, cut off and got rid of. It is not so, however, with the corresponding defective growth of the body and of the brain. There each day's deficiency is so much deducted from the size and power which might have been. It is essential, then, to true education that robust and vigorous health should be, as far as possible, maintained.

Along with health, a proper supply of good nutritious food is necessary for development. In the young, food has the double purpose to fulfil of repairing waste, consequent upon action, and providing building material for growth. The quantity of food taken, therefore, requires to be proportionately greater than in the adult, with whom it has only the one purpose to serve. Repair of waste being immediately concerned in the maintenance of health and life, makes the prior and more urgent demand upon the nutriment carried by the blood to the part concerned; and it is only on condition of this being adequately met that anything can be spared for building purposes. Waste in the brain, or in any other part, is in proportion to the action of that part; action, therefore, of any part ought to be regulated in accordance with the share of the blood supplied to it *properly belonging to repair.* Having, however, the building fund, so to speak, to fall back upon, action may be carried, without direct injury to the health, much farther than would be thus indicated, but *at the expense of growth and development,* which are impoverished to that extent. Only when the whole blood supply fails to meet the requirements of repair, does health suffer; and before that stage is reached, the building process must be at a stand still.

In the case of mental development, we have to take into account not only increase of brain substance under the conditions just stated, but also the supply of mental pabulum in the form of knowledge and ideas, without which there could be no mind, however strong and well developed might be the brain. This requirement is secured by the action of certain parts of the brain, with the aid of the senses; and this action ought to be regulated according to the general principles given above.

Exercise, or functional activity *within certain limits,* is also necessary for development. Any part of body or mind which is little exercised in youth, remains weak, and is never afterwards capable of being brought to any great degree of strength; while on the other hand, those parts which are most *exercised* bulk most largely in the capabilities of after life. The question here for the educationist seeking to promote development of the mental faculties, must be, What is the limit of exercise beyond which action becomes *strain?* The answer might be stated thus—The point beyond which the demand for repair of waste would encroach upon the share of nutriment properly belonging to the building process. But this, while it indicates the great importance of keeping mental action within the said limits, would be no help to the teacher in regulating the work of his pupils. He has still to seek for some sign by which he may recognise the point at which work must stop, if he would avoid retarding where he wishes to promote development, and weakening where he means to strengthen the mind. Where is he to look for such a sign? I have heard it argued by teachers that health is the only guide, and that all is safe so long as that does not suffer. But we have seen that, within the limits of health, the building process may be fairly starved and brought to a stand still.

When a colt is put to hard, or what for a horse of the same size would be even moderate work, it is not the health which suffers, but the future strength and power of endurance. The same effect of early strain is seen upon the human body in some mining and ironwork districts, where boys are much employed at hard manual work. Such boys do not necessarily suffer in health, or even strength, at the time. They may, indeed, be physically stronger than others of their age. But the men are much under the average in size and physique, and come early to old age. Thus it will be seen that before any indication is given, by failing health, of the abnormal strain being put upon the young brain, great and permanent injury may be done, and the seeds sown of future disease and early decay. Health, therefore, is by no means a safe guide in working the young mind.

Where, then, are we to look for guidance as to the amount and kind of mental action suitable to the child at different ages and in the ever-varying states of health and vigour and vital energy, day by day and hour by hour? Where can we look but where we find it in regard to the other voluntary activities of life—the instinctive guiding sensations placed in each part for the very purpose we have in view. So long as the teacher carries with him the interest and pleasure of the child, he is safe, and may truly cultivate the mind. His art lies in securing these for the subjects to be taught. Without them he is utterly and helplessly in the dark, and cannot move a step without danger.

I shall now endeavour to state shortly the points at which the prevailing conditions of school life come into collision with the laws and principles of health and of development.

The first point to which I shall refer is not, perhaps, so much an evil in itself as that it intensifies the gravity of other evils. I refer to the age at which children are now compelled to attend school—namely, five years. I do not agree with those writers who hold that the first seven years of life are devoted by Nature to physical rather than mental development. On the contrary, I believe that during no equal period of man's existence is so much progress made in learning and power of thought, while the restless activity of the mind is no less characteristic of the child than is that of the body. But there can be no doubt that the younger the child, the more readily is it injured by insanitary conditions and improper treatment. A child of five is a mere baby, and requires to be very gently and tenderly dealt with. When a properly conducted Infant School or Kindergarten is available, there may be no harm in sending such to school, where they may be taught something in addition to what they are constantly learning from the ordinary surroundings, and be happy enough. But it must be borne in mind that in very many country parishes there is neither Infant School nor Kindergarten ; nothing, indeed, but the one Board School with the one male teacher, whose method and manner are often far from suited to that tender age. Yet in those country parishes the Act is as imperative as in towns, and it is to the advantage of teacher and the School Board that it be enforced upon all children of school age. Infants of five years may be, and very often are, thus hailed from their mother's knee, made to walk some distance to school in all weathers, and placed for four or more hours under the almost uncontrolled rule of the teacher and his pupil teachers. Many, if not most medical men, are of opinion that it is both useless and injurious to confine children in school, and attempt to teach them school lessons, before the age of seven ; and it is subject of common remark that by the age of ten, those who began school at seven are, as a rule, fully abreast of others who began at five. One would think, then, that during those two years, during which the child would gain greatly in strength and power of resisting injury, attendance at school might be left optional with the parent, or only enforced in case of a suitable school being available. I am certain that in many cases attendance at the Board School is very injurious to these young children.

The strain of keen, anxious excitement, caused in some children by competition in the class, and the state of extreme nervousness and fear, amounting at times to absolute terror, in which others are kept by the manner of the teacher, and his method of maintaining order and discipline, and enforcing the learning of lessons, cannot fail to have an injurious effect upon the nervous system. In my practice, I have had reason to ascribe several cases of severe illness to this cause ; and I have at present under observation several children who are always more or less ill—nervous and irritable, with poor appetite, broken sleep, &c.—while attending school, and who soon get well on being removed, the cause being, I believe, as above stated. Chorea, hysteria, *et hoc genus omne,* will be a natural development from such a state of things.

The most effective agency in maintaining robust and vigorous health, so necessary to full development, is plenty of free and hearty exercise in the open air. Young people are never so well as when they spend nearly the whole of every day in this way, as during their midsummer holidays ; and the health and vigour of the system deteriorate in proportion as they are deprived of it. As a rule, little or no provision is made for this in school life. It is true that attached to most boys' schools there are playgrounds, where some of the pupils may at times be seen playing football, cricket, &c.; but whether any or what time can be spared for such exercise depends, not upon the requirements of health, but upon the demands of school work. The average school day for boys may be taken as five or six hours in school, and about two hours of evening work. During the long day, this may leave two or perhaps three hours for out-door exercise. As the days shorten, on the approach of winter, the school hours remain the same, and the allowance of out-door exercise gets small by degrees, till in mid-winter it disappears altogether. Then there are numerous other encroachments upon the small modicum of playtime allowed. A favourite form of punishment is to keep in the pupils after school hours; another to give extra tasks, which have a similar effect. Extra classes, such as music, dancing, &c., are usually taken during playtime; while preparation for the Inspector's visit, for examinations, and for prize and bursary competitions, frequently occupy every minute of the day not required for ordinary school work, and often a considerable part of the night as well. It will be said that the boys have the whole of every Saturday to themselves, which makes up for the week's confinement. But the fact is that the confinement of the week takes away, to a great extent, the power of properly enjoying the Saturday. It makes them listless and out of condition, and they have little inclination for vigorous exercise. Daily exercise is required almost as much as daily food; and six days' starvation with a Saturday's "burst" is as contrary to physiological principle in the one case as in the other. In many boarding schools for girls, no allowance whatever is made for this requirement of health, the formal walk which goes by the name of out-door exercise being a mere travesty of what is required. Indeed, anything like free and vigorous action, either in the house or out of it, is generally forbidden as unladylike. Need we wonder at the "Little Health of Ladies?"

Thus, in our efforts to develop the powers of man, we deliberately set at naught the most elementary principles of development. It is as though, in attempting to cultivate a plant, we were to place it in a darkened room, and ply it copiously with artificial manure. When serious symptoms of weak health show themselves—when the appetite fails, the cheeks get pale and thin, and the spirits dull and listless, the parent may take alarm, and the doctor be called in. His prescription is, to be taken from school and be a great deal in the open air. This generally succeeds, in course of time, in restoring the vigour and buoyancy of youth, and the parents think that all is right again. But the evil is not recovered from, and never will be. For months, development has been languishing, and probably not half the progress has been made that might have been. The time thus lost can never be made up, but remains as so much deducted from the period of development, resulting in a corresponding loss to future capacity and strength.

The only other point of antagonism between school life and the principles of health and development to which I shall refer, is the great tendency to *strain* the young mind. This, indeed, seems to be the chief object aimed at by the various means of stimulation employed, as it is difficult to understand the need for them on any other grounds. The young mind, like the young body, is strongly inclined to activity. If left alone, it would never be for a moment idle, except during sleep, and it readily acquires all the information which comes within its reach, thus learning an immense deal more than is obtained from school lessons. One would think that this strong tendency to exercise and to learning, analogous to the well known tendency to bodily exercise and the appetite for food, would be sufficient for the teacher's purpose, only requiring to be assisted and directed in the proper direction. Certainly it is only by keeping within this, the natural and only guide, that strain can be avoided. Yet the whole scholastic machinery of prizes and punishments seems directed to force on mental action in opposition to, and beyond the indications thus given by, Nature for the conduct of this natural process. If "even" teachers kept the matter in their own hands, and dictated the exact amount of work to be done, there might be some guarantee for its not being excessive. Practically, however, the prizes, bursaries, and scholarships serve as an inducement, not to do a certain amount of work, but to test who can work hardest and longest, and strain their mental powers to the greatest extent. The system of punishments acts in the same direction, though not to the same extent; but the distress of mind occasioned, especially with nervous children, is both a cruel and an injurious infliction upon child life. The need for either prizes or punishments in connection with school lessons, has the same significance as it would have in the case of the other voluntary activities—bodily exercise and the taking of food—and means something unsuitable in kind or quantity, or in both. We know well what would be the effect of forcing food or physical exertion upon the child, in opposition to or beyond the instinctive guides. On what ground is such treatment deemed applicable to the mind? Or what reason is there to suppose that mental strain, carried as it often is to the very verge of endurance, can be other than highly injurious to the organ concerned?

In conclusion, I would point out the fallacy of taking the mortality statistics as an indication that the high pressure now being applied to school work is not injurious. We may freely admit that the sanitary conditions of school life, as regards fresh air, warmth, cleanliness, &c., are a vast improvement on the conditions under which many wretched little vastrals in large towns existed previous to being taken hold of by School Boards; and this may tend, as pointed out by Mr. Mundella in Parliament, and repeated by other defenders of the Act, to a considerable reduction of juvenile mortality. But I think no one will venture to assert that *school work* tends to prolong life or prevent illness; and there is nothing in the mortality statistics to prove that it may not be having a very injurious effect upon the brain and mind. It takes a great deal to kill a child, and there are many degrees of injury short of death which may be caused by strain upon the growing brain. May not the great prevalence in this country of insanity, with the allied phenomena of suicides—the increase of which seems to be keeping pace with the educational high pressure of the day—be partly owing to this cause? Is it not a result much akin to the early breakdown of the horse, following upon hard work of the colt? Do not, also, the reported unsatisfactory results of the competitive system of making appointments to the Civil Service, and the after career of senior wranglers and other college and school prizemen, point in the same direction, and show that the vast machinery now so actively employed throughout the country for urging on hard study in youth, is in reality a means of cramping and narrowing the intellectual powers, and preventing the free expansion of mind for which nature has provided? When school teaching is placed upon a sound physiological basis, and all ulterior and adventitious motives for mental work are dispensed with, then may the Education Act be worked with confidence and safety. Until then, the good effected is sure to be largely mixed with evil; and it is a very open question whether the country will benefit or suffer most from its operation.

IDIOPATHIC PERICARDITIS.

By R. B. MACPHERSON, M.B., Cambuslang.

In most of the standard works on medicine we find idiopathic, or uncomplicated, pericarditis, referred to as an exceedingly rare affection; indeed, in some we find no mention of it whatever. Niemeyer, in his *Text Book of Practical Medicine,* says:—"It is extraordinarily rare for this malady to attack previously healthy persons as an independent, isolated disease." Latham, in his work on *Diseases of the Heart,* says: —"In short, of pericarditis, not in alliance with acute rheumatism, I do not know that a single uncomplicated case ever fell under my observation." Again, Dr. Balfour, writing in *Quain's Dictionary of Medicine,* says:—"Pericarditis is occasionally idiopathic, but so-called idiopathic pericarditis is usually associated with pleurisy."

In the *Glasgow Medical Journal* for September, 1878, Dr. Walter Hunter, of Mearns, reported a case of idiopathic pericarditis, attended by extensive effusion, and ending in adhesion of the pericardium. Now, while I would acknowledge that such severe forms of the disease are very rarely uncomplicated, I am by no means certain but that in its slighter forms, such as the case which has come under my notice, idiopathic pericarditis is much more common than we consider, and that in many cases it remains undetected from the slightness of its symptoms, the shortness of its duration, and the want of careful physical examination. Two days before the patient whose case I have to report came under my notice, the symptoms of which she complained were attributed, by a medical practitioner, to dyspepsia.

C. R., æt. 16, was brought by her mother to consult me on the 8th July, 1883. She has always had fair health, with the exception of an attack of periostitis of right tibia, six years ago, said to have been caused by a kick. She has not menstruated yet. Family history is good—no rheumatic tendency. During the past two and a half years she has been at work in a weaving factory. For two or three weeks past, her appetite has not been very good, and she has not seemed quite so well as usual: but she did not complain till four days ago, when she began to feel slight breathlessness on walking quickly, or on going up a stair. Yesterday she gave up work, as the breathlessness was more troublesome, and she felt also slight pain in the epigastric and præcordial regions. With the exception of pallor of complexion, there was nothing striking in her appearance. Pulse 90, rather feeble; respirations, 24, shallow. She did not complain of the breathlessness, except on exertion, and the pain was not at all severe, but was increased by lying on the left side. No pain of the joints; no evidence of pulmonary affection; urine normal. On listening over the cardiac region I was surprised to hear a very well marked superficial, rough, grating murmur. It was heard more or less distinctly over the whole cardiac area, but loudest about mid-sternum, and was not heard in the vessels of the neck. It accompanied both systole and diastole of the heart, and was not affected by cessation of breathing. Cardiac dulness not increased, and, so far as could be made out, no endocardial murmur present.

I ordered the patient to be kept in bed, and a fly blister to be applied over the cardiac region. During the following week, there was little change in her general condition. She scarcely complained at all while lying quietly in bed, except of being unable to lie on the left side. There was no obvious dyspnœa, nor distress of any kind. The friction sound continued, with scarcely any change, for three or four days. It then gradually diminished in intensity, and had entirely disappeared on the eighth day after my first seeing the patient. There was no increase of cardiac dulness, nor any other evidence of effusion, and the heart sounds were left perfectly pure after the disappearance of the friction sound. The temperature throughout never exceeded 99·5°. The patient was kept in bed a week after the disappearance of the friction sound, and was then allowed up gradually. She expressed herself quite free of pain and breathlessness, and has remained well since.

I have no doubt that this case was an example of a local and limited inflammation of the pericardial surfaces, attended by very little, if any, effusion, and ending in the production of local lymph patches, or local adhesions of slight extent. Most writers on pericarditis, however, speak of the formation of these "lymph patches" as being unattended by clinical symptoms or physical signs, and as being quite unrecognisable during life. Niemeyer says:—"The growth of villi, and the formation of maculæ albidæ, are not recognisable during life, but are mere accidental *post mortem* discoveries." And G. W. Balfour, in *Quain's Dictionary of Medicine*, says:—"Milk spots are the result of some trifling local pericarditis, running its course without symptoms, and of no clinical importance."

In a paper on pericarditis, in the *Edinburgh Medical Journal* for 1859-60, Dr. W. T. Gairdner holds strongly the opposite opinion. He says:—"It is commonly assumed, by writers on pericarditis, that those curious and very common sequelæ of disease, called 'white lymph patches,' and 'milk patches,' are practically soundless, as well as devoid of symptoms. This is a very convenient doctrine, and leads to great simplification of the subject of cardiac murmurs; but I am quite sure that it is not always in accordance with fact. On the contrary, although the paucity of symptoms in these cases is a great obstacle to their recognition during life, I have again and again detected, quite accidentally, over the cardiac region murmurs which, to the best of my knowledge and belief, could have no other probable origin than these white patches; and I have in a few instances traced these murmurs also to their cause, by a *post mortem* examination, which has shown both the existence of the white patches and the non-existence of any other recognised cause of murmur."

Dr. Gairdner found that lymph patches, or mere threads of adhesion, were present in not less than a third of all the patients who died in the Edinburgh Royal Infirmary, and he came to the conclusion that the formation of lymph patches, as the result of slight and local irritation, is one of the commonest of morbid affections. Is it not, then, probable that in a considerable proportion of cases the formation of such lymph patches or adhesions is attended by symptoms and physical signs such as were present in the case I have reported, and that such cases are frequently not recognised?

CURRENT TOPICS.

Sir Joseph Lister.—There are many in Glasgow who experienced a feeling of personal satisfaction at the news that Lister was at last to receive a baronetcy in recognition of his position as a scientific surgeon and the originator of the Antiseptic System. It was in the session 1865-66, that Lister began, in Ward XXIV of the Royal Infirmary of Glasgow, that system which has immortalised his name. The case was one of compound fracture of the leg, and the dressing was the patient's blood mixed with carbolic acid, forming a paste which occupied the wound and was covered with a cap of tin. Glasgow had thus the honour of seeing the first experimental efforts towards the scientific treatment of wounds on the theory that most of the mischief to which they are liable is due to putrefactive changes occurring in them. This theory has been persistently held up and insisted on as the essential basis of Lister's system, and, in all the changes which the material used by Lister himself in his treatment has undergone, no change has taken place in the theory. He has never ceased insisting that without the theory the treatment is nought, and some people have got tired of this insistence and have endeavoured to separate the treatment from the theory. To do this is to convert Listerism into a mere piece of empiricism, and those who have had personal experience of the purely scientific turn of Lister's own mind, will have the feeling that this is to depart entirely from his spirit.

There are some readers of this *Journal* who were students in Glasgow University with Lister, and who attended his class even before the antiseptic period. They will remember his teaching as the inspiring expositions of a master. With a hesitating manner and slow diction, so that every one could note down almost every word, he gave those who followed him a peculiar insight into the physiology of disease. The meaning was always clear and the whole of the remarks had a definitely logical progression. The commonest things were regarded as matter for scientific study, and we were taught to cultivate what he sometimes called the physiological eye, looking always beneath the surface and endeavouring to picture the processes going on in the living tissues although invisible. The teaching was certainly dogmatic, and little reference was made to any authorities except John Hunter and Mr Syme, but yet the tendency was not narrowing, and a method of regarding the vital processes was cultivated, which was felt to give the key by which the whole body with its various healthy and morbid actions might be explored. Those who sat on the benches and listened to the courses of systematic lectures will perhaps appreciate these remarks, which may bring back to them pleasant memories and revive impressions of the happy spring time of learning. In his ward teaching Lister was no less inspiring. The case was not merely one to treat on the ordinary principles of surgery, but was a problem which the surgeon had to study. Every circumstance connected with the case was a fact which nature was presenting to us, to enable us to understand her workings, and often an apparently trivial point was dwelt upon at what the heedless regarded as a weary length. All Lister's conclusions have been come to by long and patient labour, and have therefore the elements of stability in them.

Since the Glasgow days when the antiseptic system was in its infancy many changes have been made in the method of carrying it out. There were many changes in these days, and the whole aim of Lister's work has been by experiment and invention to perfect the material and find what is most adaptible to the purpose. The actual material whose use is known to the profession is but a small portion of what has passed through his hands, for much that promised well at first was given up after a patient trial. The changes which have taken place may be regarded as developments of the method, as means by which advance has been made to higher things.

So much has Lister's antiseptic system overshadowed his other work that the latter seems to have been in great part forgotten. But those of us who listened to his systematic lectures will remember his researches on the Coagulation of the Blood, and on Inflammation as having been published long before that system was thought of. The observations on Inflammation were of special value and have been too little remembered by subsequent writers.

It is seldom, we believe, that any man receives during his life-time such universal recognition as Lister has obtained from all civilised nations, and the extent of this recognition is an indication of the unity of our profession in every race. Among the honours conferred on Lister we find Denmark, Germany, France, Silesia, Finland, and Norway represented, as well as the cities of Munich, Leipzig, Vienna, Buda-Pesth, Dresden, Amsterdam, Petersburg, Paris, Brussels, Philadelphia, London, Oxford, Cambridge, Dublin, Edinburgh, and Glasgow. It may indeed be said that in foreign countries Lister's system has obtained a much more universal acceptance than at home, where its merits will only be fully recognised when its author is no longer with us. We feel sure that our readers will join with us in the hope that Sir Joseph may long live to enjoy his well merited honours.

Discussion on "Catheter Fever" in the Medico-Chirurgical Society.—This very interesting discussion was held at the meeting on 11th January, and a full report of it will appear in our next issue. There is some talk, we believe, of a renewal of the discussion with the assistance of gentlemen from a distance. We hope that this may be carried out as we feel sure that there are many even in Glasgow who have still something to say on the subject.

Discussion on Albuminuria at the Pathological and Clinical Society.—As formerly mentioned this Society has arranged for a discussion on "Albuminuria, its Pathology and Clinical significance," to be held on the evenings of 12th and 19th Feb. We learn that Dr. Roberts, Manchester; Prof. Greenfield, Edinburgh; and Prof. Hamilton, Aberdeen, are likely to be present and take part in the discussion. A considerable number of Glasgow members will also speak, and it is not improbable that the discussion may extend over a third evening. The meeting will be open to medical men in the neighbourhood, but those who wish to be present are requested to communicate with Dr. Newman, Secretary.

Fever Clinique by Dr. Allan, Belvidere.—We are pleased to learn that the great field for clinical instruction afforded by the Fever Hospital, Belvidere, is to be made available, and that a course is to be conducted there during the summer by the accomplished physician, Dr. J. W. Allan.

REVIEWS.

Elements of Surgical Pathology. By Augustus J. Pepper, M.S., M.B. Lond., F.R.C.S. Eng., Surgeon to St. Mary's Hospital, London. London, Paris, and New York: Cassell & Co., Lim. 1883.

Except the works of Paget, Billroth, and Walsham, there are no books in the English language exclusively devoted to the subject of surgical pathology, and these, though admirable in their way, can hardly rank as suitable for the ordinary run of students. Still further, it is some time since these works appeared, and with the rapid strides made in recent years in this particular branch of pathology, there is great need of a work which would deal with the subject up to date. A book, therefore, which purports to introduce some of the most recent investigations, and to place these with the whole subject in a suitable form for the use of students, is to be greeted with pleasure. Another object has been held in view by the publishers, of introducing the material in such a manner as to render its size, as well as its cheapness, compatible with the student's pocket. With this latter consideration, the little volume before us fully complies. What, however, mostly interests us is the material which it contains, and more especially as to how far it is likely to meet the requirements of the medical student. Upon the first point, Mr. Pepper has carefully introduced every recent consideration upon the subject; and where doubts still exist, and theories alone supply the place of needed facts, the various views of different observers are impartially placed before the reader, who, in many instances, is permitted to draw his own conclusions. The *required* size of the book necessarily forbids any discursive elaboration of these points, however much of didactic value such would be. As to its second point—its value to the student—we must confess ourselves somewhat disappointed; and here, not in the method in which the work is executed, but in the often obscure conciseness which the size of the book has, almost of force, entailed. To illustrate our meaning—What, it may be asked, is the student likely to understand by meeting, perhaps for the first time, with such terms as "caco-plastic lymph," "ectasia," "indifferent cells," "vaso-formative cells," "vascularised lymph," "inflammatory embryonic tissue"? These, and many others, occur without definition, as if indeed the reader were perfectly familiar with the subject, rather than a seeker after information. No doubt the names themselves will in many cases be sufficiently suggestive to some students, but to the majority they will be unintelligible. Again, throughout the text one cannot fail to be impressed with an abruptness of style, suggestive rather of the disconnected jottings of a student's note book than of the needed clearness and legible continuity of a manual. Thus we fear the little volume must prove both difficult to read and difficult to remember, and we can only regret that Mr. Pepper has been forced to produce something convenient for the pupil's pocket rather than to satisfy—as he could well have done—his intellectual requirements. If the book is still to maintain its "pocketable" size, a glossary would add much to its value.

The Australasian Medical Directory and Hand-Book. Edited and Compiled by Ludwig Bruck. London: Baillière, Tindall, & Cox. 1883.

We have to congratulate our Australasian brethren that they have so far attained to the position of a united profession as to possess a "Medical Directory," the first issue of which is before us.

The first part of the work contains an "abstract of the principal laws affecting the medical profession" in Fiji, New South Wales, New Zealand, Queensland, South Australia, Tasmania, Victoria, and Western Australia. "The second division," in the words of the Introduction, "comprising a General Gazetteer of nearly 1,000 post towns in Australia, Tasmania, New Zealand, and Fiji, together with a Local Directory, will be of particular value to any practitioner desirous of changing his residence, as it shows him at one glance all the particulars he may require to enable him to choose a fresh field of labour.

"The Editor feels confident that the *Australasian Medical Directory and Hand-Book* will also be well received by the profession abroad, especially by those of its members who may have the intention to settle in any of the Australasian colonies, as it will give them the fullest information as to the state of the medical profession in Australia and New Zealand."

The work contains a list of the medical and scientific societies, and the regulations of the Colonial Universities. There is a section, also, on the Australasian colonies geographically defined. Lastly, a list of the periodicals devoted to medicine published in the English language throughout the world, is also given.

Altogether, great enterprise and practical sense are shown in this publication.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM PROFESSOR M'CALL ANDERSON'S WARDS.

I. CASE OF PERITONITIS COMPLICATED WITH ABSCESS BELOW THE EAR.—[Reported by W. L. Strain, M.B.] A gardener, 23 years of age, was received into the Infirmary on 17th October, 1883. He was so ill that it was impossible to get a full history of his complaint; but it seems that it set in during the night three days previously, when he was awakened by a severe pain in the right iliac region, which soon became diffused over the whole abdomen, and was accompanied by a good deal of distension. On admission he was in a state of extreme debility; pulse 120, weak and thready; temperature 102·4° F.; his abdomen, which was very decidedly distended, was so exquisitely sensitive that he could not bear to have it touched, and he lay upon his back with his legs drawn up, his countenance having a worn and suffering appearance. Vomiting was a very constant and annoying symptom from the first, scarcely anything lying on his stomach, and his bowels had not been moved for several days.

He was fed upon iced milk, 10 grains of Dover's powder was prescribed every four hours, and fomentations were applied to the abdomen.

On the 19th poultices were substituted, the abdomen being smeared with a mixture of belladonna and glycerine, and as the sickness was very troublesome the Dover's powder was replaced by half grain morphia suppositories.

Next day, 20th October, as the sickness still persisted, a grain of solid opium was given by the mouth every 4 hours instead of the suppositories; and as he complained of the weight of the poultices, fomentations, sprinkled with laudanum, were substituted.

23rd October.—No improvement is manifest, the sickness and vomiting being even worse, and the pulse 120, weak and thready; but the temperature was normal in the morning, and 99·6° in the evening. The opium by the mouth was stopped, and subcutaneous injections of morphia commenced, beginning with $\frac{1}{4}$ grain night and morning, the dose to be gradually increased until the pain was entirely subdued. His diet still consisted of iced milk, with ice to suck, and a teaspoonful of brandy was ordered every hour.

24th October.—Although the pain was much relieved the irritability of the stomach continued; therefore, the milk was stopped, and he was fed by enemata of Carnrick's beef peptonoids three times a day, when the vomiting immediately disappeared.

28th October.—The irritability of the stomach has not reappeared, and the abdominal pain is comparatively slight; but since the commencement of the enemata there has been a tendency to diarrhœa with some tenesmus. Morphia suppositories were therefore administered, and a little milk by the mouth.

On 8th November, the diarrhœa being still troublesome, and sickness keeping away, the nutritive enemata were stopped, and milk and milk foods cautiously given by the mouth; also, in addition to the morphia hypodermically, lead and opium pills were given at intervals.

He complained to-day for the first time of a painful swelling below the left ear. This swelling gradually increased in size until it reached that of an orange; it became very painful; the skin over it was tense and shining, and he was unable to open his mouth. The temperature since the 23rd of October had rarely much exceeded the normal, and there was no notable rise owing to the swelling, the highest being a fraction over 100° F. This swelling proved to be a large abscess which was opened antiseptically on 14th November.

From this time onwards his recovery was almost uninterrupted. His diet was cautiously improved, and his bowels carefully regulated. The morphia was discontinued about the first week in December, and for some time previous to this he had been having only one injection of gr. $\frac{1}{6}$ to gr. $\frac{1}{4}$ daily. On 16th December powders of lactopeptine and bismuth were given after each meal.

4th January, 1884.—To-day he was dismissed, feeling, as he says himself, "better than he ever did in his life," and complaining only of some numbness of the left ear and left side of the face.

Remarks by Dr. M'Call Anderson.—Considering the acuteness of the attack, the severity of the symptoms, the persistent vomiting, and the formation of a large abscess, the result of this case must be considered highly satisfactory. The greatest difficulty in dealing with it was due to the irritability of the stomach, for whenever we fed the patient exclusively by enemata, while the sickness was arrested, diarrhœa commenced, and *vice versa;* and it was only by the most careful alternation of the two methods of feeding that success was at last attained.

But what particularly interested me in this case was the occurrence of abscess below the ear, as this was the second case of the kind with regard to which I had been consulted in 1883. This was the case of a young lady, 13 years of age, whom I saw repeatedly in the month of March in consultation with Dr. M'Jannet of Lennoxtown. Hers was a most uncontrollable attack of peritonitis, on the twentieth day of which pain began to be complained of below the right ear, with swelling, which rapidly increased, and was accompanied by an erysipelatous blush on the right side of the nose, and which terminated fatally.

The only way in which I can account for the formation of these abscesses is by supposing that they were of a septic nature; but it is somewhat curious that in both cases they should have occupied the same locality.

II. CASE OF ACUTE TUBULAR NEPHRITIS TREATED WITH SKIMMED MILK.—A mason, 24 years of age, who was much exposed to vicissitudes of weather, and who was frequently in the habit of putting on clothing which had not been thoroughly dried after being wet on the previous day, came into the Infirmary on the 19th November, 1883, complaining of swelling of his face, hands, and legs, with cough and expectoration of about a week's duration.

While at his work his fellow-workmen told him that his face was swollen, but feeling very little wrong, he paid no heed to them, and continued at work till the 5th inst., when he found his feet and legs considerably swollen, and next day his hands and arms became similarly affected and stiff. During this time, too, he noticed that his urine was very scanty and high coloured, although his bowels were regular.

On admission there was very marked œdema of the face, legs, and arms, and cough was very troublesome, and accompanied by some frothy and mucous expectoration, while pretty abundant musical râles were heard throughout the chest. The cardiac sounds were normal, with the exception of slight accentuation of the second sound at the base. There was no fever and no pain or tenderness in the loins. The quantity of urine passed was 30 ounces in the 24 hours; it was high coloured—sp. gr. 1028—contained a large amount of albumen, and there was a copious deposit in which tube casts were found in abundance, many of them of the epithelial variety.

The only medicine given was ʒi. of compound powder of jalap on admission, which moved the bowels freely, but did not ameliorate the symptoms. Accordingly (on 14th Nov.) he was fed exclusively upon skimmed milk for 8 days, to the extent of from 11 to 14 pints daily. The urine immediately increased in quantity, and on the fourth day reached 220 ounces, by which time the dropsy had almost entirely disappeared; no tube casts could be found, and the urine only contained a slight amount of albumen. On the eighth day from the commencement of the treatment (22nd Nov.) the dropsy and albuminuria had entirely disappeared.

The last examination was made on the 18th December, 1883, when the amount of urine passed was 82 ounces, the sp. gr. 1020, and it continued altogether free of albumen and casts. The bronchitic symptoms had entirely disappeared, and the patient was dismissed early in January.

Remarks by Dr. Anderson.—The treatment of tubular nephritis by means of skimmed milk often yields the best results, if we see the patient sufficiently early, and can induce him to drink sufficient quantities of the milk, and is thus opposed to the view, held by many, that diuretics are inadmissible in cases of acute tubular nephritis.

As a further illustration of this method of treatment, the following case may be mentioned:—On the 16th of May, 1878, I was consulted by a lad 14 years of age, who had been treated by me two years previously for an attack of tubercular peritonitis, which is elsewhere reported.* He had well marked and increasing œdema of the face, of four days' duration. The urine was loaded with albumen, and he had the other usual symptoms of tubular nephritis, which it is unnecessary further to mention. He was kept in bed and fed on skimmed milk, which he took in large amount, but the exact quantities were not measured. The following table shows the result of the treatment:—

Diet.	Date.	Urine.	
	May 16, 1878,	40 ounces.	
	„ 17, „	152 „	
Cup of tea and toast in addition,	„ 18, „	224 „	
	„ 19, „	168 „	
	„ 20, „	140 „	
Chicken soup and less milk,	„ 21, „	88 „	
Fish; out of bed for a little,	„ 22, „	88 „	No albumen.
	„ 23, „	96 „	Dropsy gone.
Tinct. Ferri. Mur. ♏.x. thrice daily	„ 24, „	60 „	
	„ 25, „	56 „	
	„ 26, „	70 „	
	„ 27, „	40 „	Keeps quite well.

* *Lectures on Clinical Medicine* London: Macmillan & Co. P. 152.

MEETINGS OF SOCIETIES.

PATHOLOGICAL AND CLINICAL SOCIETY.

Session 1883-84.

Meeting IV.—*8th January, 1884.*

The President, Prof. M'Call Anderson, *in the Chair.*

Dr. Perry showed a specimen of a urinary bladder where there was very extensive sub-mucous ecchymosis, with one of the kidneys from the same case. The preparation was obtained from a man who had lain out for several nights at the New Year holidays, and who died shortly after his admission to hospital. This case was of interest for comparison with that to be brought up by Dr. Finlayson.

Dr. J. Lindsay Steven showed a specimen illustrating impaction of a foreign body in the pelvis of the kidney. The specimen was one about which but very little could be said, as it was to be regarded more as an accident or a curiosity than anything else. The kidney was obtained from the body of a man who had been admitted to the surgical wards of the Western Infirmary, suffering from fracture of the skull, and who died shortly after his admission, so that no history throwing any light upon the condition at present under consideration was obtained. The organs generally presented healthy characters; but upon proceeding to make a section through the right kidney, a large quantity of pure yellow pus made its escape. Upon laying open the organ, the pus was found to have been collected in a series of large cavities, chiefly in the upper part of the organ, and lying in the long diameter of the pelvis was observed a long black bristle, or wire-like looking body, about 1½ inches in length. That this had not accidentally (*i.e.*, during the course of the examination) obtained its position *in situ* was at once proved by the fact that its lower extremity passed right through, and was immovably fixed in, a branched phosphatic calculus which had been deposited round about it. The branches of this calculus passed into the calyces in its neighbourhood. The foreign body lay exactly in the direction assumed by a probe when passed up the ureter into the pelvis. The bladder contained a little purulent urine, but was healthy, as was also the right ureter, which was dissected out in its whole length. A small portion snipped off the foreign body showed it was not metallic; on placing the little bit in glycerine, and trying to tease it with needles, it split very readily into longitudinal pieces, which, upon microscopic examination, were seen to be fibrous, and of a reddish colour. It was obviously an animal product, probably a bristle. The body was about the thickness of a soda water wire. It must have first obtained entrance to the bladder, and then made its way up the ureter to the pelvis of the kidney.

Dr. J. Lindsay Steven also showed a specimen illustrating complete occlusion of the innominate artery, arising from thrombosis, the result of extreme atheroma, and read the following notes:—

The specimen exhibited illustrates a somewhat unusual result of a very common affection, and the condition which it presents is, I think, of considerable practical importance from several points of view. The preparation was obtained from the body of a man who was treated for several months in the wards of Professor M'Call Anderson, to whose kindness I am indebted for the following clinical notes:—He was admitted to the Western Infirmary upon the 28th June, 1883, complaining of pain in the præcordial region; and his whole illness seems to have dated from about six weeks before his admission, as previous to this time he had never suffered from any illness. He was suddenly seized, while at work, with a cardiac pain having all the characters of angina pectoris, and after this first attack the seizures increased in number and severity, causing him to leave his employment and seek admission to the hospital. The attacks were always most frequent and severe after exertion of any kind, and lasted generally from five to fifteen minutes.

When about 20 years of age he suffered from venereal disease, although it is difficult to be sure whether it was syphilitic or not. In this regard it should be noted that at one time he suffered from nocturnal rheumatism in the tibiæ, and that numerous, small, white, depressed cicatrices were scattered over the back.

A loud v.s. and v.d. (double) murmur was heard at the base and down the sternum, and this, along with dilated hypertrophy of the left ventricle, pointed to extreme aortic obstruction and regurgitation. There was obscure pulsation in the suprasternal notch; the right pulse was exceedingly feeble, as compared with the left; and the superficial vessels were atheromatous. These signs, along with the clinical history, pointed to the probable presence of an aneurism of the first part of the aorta or of the innominate.

Upon the 23rd Dec., 1883, while in the bath room at stool, he suddenly died, falling forward on the floor. There was no external hæmorrhage. On the 25th, I made an examination of the body, and obtained the specimen which I now beg to bring under the notice of the Society.

The preparation consists of the whole of the thoracic aorta, including the vessels at the root of the neck. It will be seen that the entire vessel is the seat of the most extreme atheromatous change, presenting here and there very typical calcareous plates. The degeneration is not confined to the aorta itself, but extends down into the common iliacs, and up into the vessels of the neck. At one part of the descending portion a pouch-like dilatation is present, but there is nothing like aneurism anywhere. At the time of the *post mortem*, on attempting to pass a probe through the innominate artery, it was found to be quite impervious, and to be practically converted into a solid mass of tissue. The vessel is certainly not increased in size, although it is doubtful if it is at all diminished. After hardening in alcohol, a section was made through the posterior wall of the vessel, and the occlusion may now be seen to be due to the plugging up of its lumen by a solid mass of rather pale, fleshy looking tissue. The plug is closely applied to the vessel wall all round, although it does not seem, in some situations at least, to be very firmly adherent to it; and it extends from a little above the origin to just beneath the spot where the subclavian is given off. It will be noted that the orifice of that vessel is very small, and that the carotid beyond the obstruction is exceedingly atheromatous and calcareous. It will be observed, too, that the left carotid is somewhat dilated at its origin.

A small longitudinal portion, including vessel wall and plug, is subjected to microscopic examination, with the following result: The internal coat is observed to be exceedingly thickened and atheromatous, and to present in several localities cavities with ragged, broken down walls (atheromatous abscesses). The plug presents the appearances of much compressed fibrin, without any sign of organisation, except that here and there colonies of round cells are observed, as if the process were commencing.

The left ventricle was much dilated and hypertrophied, and the aortic curtains were quite incompetent. The valves were much contracted, and one of them was so adherent that its movements were very extremely impaired. One coronary artery was unchanged, but the orifice of the other was narrowed. Just above the level of the free margins of the valves the wall of the aorta was so hard, that it felt as if a whalebone ring had been placed in it.

Concerning the condition of other organs little need be said. The kidneys presented some degree of cystic change, and showed several cicatrices on their surfaces; the liver was slightly hyperæmic.

As I have said, the condition illustrated by this specimen is very unusual, and this statement will, I think, be agreed to when the large size of the affected vessel and its close proximity to the heart are considered. It is not at all uncommon, as has been frequently pointed out, to find the smaller arteries, very specially those of the brain, occluded as the result of atheromatous disease, for the obvious reason that the vessels are so small and so far removed from the heart that it takes but little to obliterate them. It is very different, however, with the aórta, and in none of the works that I have as yet been able to examine can I find obliteration of the aorta, or of one of its large branches, mentioned as one of the results of atheromatous disease. In the last edition of Cornil et Ranvier the remarks on the "obliteration of arteries by endarteritis and thrombosis" are confined to those of "middle or small calibre."* In an old edition of Cooper's *Surgical Dictionary* † I find 4 cases of obliteration of the aorta mentioned, and of these probably 3 were congenital and of the same nature as the 38 cases of congenital stenosis of that vessel which Peacock has analysed.‡ In the fourth the obliteration was concerned in the healing of an aneurism. Dr. Wyeth of New York mentions a case of obliteration of the popliteal from atheroma,§ and Dr. Newman informs me that he has lately met with the condition in the femoral. These are the only cases of a somewhat similar nature that I have been able to find; but still, without a search through the recent medical journals and the *Transactions of the Pathological Society of London*, it would be too much to

* *Pathological Histology.* Eng. Trans. London, 1882. P. 512.

† Seventh edition. London, 1838. P. 234.

‡ Quain's *Dictionary of Medicine.* London. P. 64.

§ *International Encyclopædia of Surgery.* Ashhurst. London, 1883. P. 352.

assume that the present case is unique. It may safely, however, I think, be taken for granted that it is one of exceeding rarity, and as such deserves to be recorded as illustrating a possible result of atheroma which has not before been sufficiently insisted upon. The case is also of interest as showing that the right arm may be sufficiently supplied with blood after complete occlusion of the innominate, and possibly, although the conditions are very different, it may be of some service in considering the question of ligature of that vessel.

Before concluding this communication I would like to refer to one other point—viz., the mode of origin of the condition and the present state of the blood clot. There can be little doubt that the occlusion has arisen from the gradual coagulation of blood upon an extensively diseased vessel wall, and it therefore becomes important to inquire into how far organisation can take place under such circumstances. In artificial ligature of vessels in healthy dogs organisation occurs in from three to five weeks; in our case, although the date cannot be accurately fixed, the vessel has probably been plugged for several months. My friend, Dr. Newman, informs me that, from observation on a case of occlusion of the femoral, he is of opinion that the presence of atheroma seriously interferes with the replacement of the thrombus by organised tissue. From microscopic examination of the present case, I am quite inclined to agree with Dr. Newman in this, although, as I have said, here and there round cells were noticed as if an attempt at organisation had been going on. This point also may be of some importance in reference to the ligature of arteries.

Professor Gairdner said it was not obvious to him, from looking at the specimen, that the condition was the result of atheroma, and asked Dr. Steven how he supposed so.

Dr. Steven replied that from the microscopic examination he was able to see where the internal coat of the vessel terminated and the clot commenced, and that the internal coat presented the usual appearances of atheroma. Considering the extensive atheroma in other parts it was not unlikely that the occlusion had arisen from it.

Dr. Newman wished to make a few remarks on the subject of the organisation of blood clot within arteries, the seat of atheroma, to which Dr. Steven had referred in his paper. He had lately investigated a case of thrombosis of the femoral artery, where, in one situation, there was marked thickening of the internal coat from atheroma. In this case, although the condition had been present for 8 weeks, no attempt at organisation had occurred, and the reason was that the nutrition of the vessel wall was very seriously interfered with by the disease. In healthy dogs he had seen, after ligature, the clot completely organised in from 4 to 6 weeks.

Dr. Joseph Coats asked what was the cause of death in the case.

Professor George Buchanan inquired if anything had been made out as to the condition of the anastomosing circulation: what was the condition of the pulse; and if there had been any coldness or loss of power in the arm.

Dr. D. C. M'Vail said that, as the case had for a period of 6 weeks been under his charge in the Infirmary, he could reply to some of Dr. Buchanan's inquiries. The pulse to the finger and the sphygmograph was quite distinct, but feebler than that on the left side, and it presented distinctly aneurismal characters, the up-stroke being very slanting. The opinion he arrived at was that the patient was suffering from aneurism, most likely of the innominate artery. Such a thing as occlusion had never occurred to him. There was no coldness of the right arm, which was used just as well as the other one.

Professor M'Call Anderson said that during life he had made out a V.S. and V.D. murmur at the base, and distinct atheroma of the superficial vessels. There was also very distinct angina. He had suspected aneurismal dilatation from (1) the pulsation in the suprasternal notch; (2) the slight dilatation of the veins of the neck; and (3) the marked difference in the radial pulses. The condition must have occurred long before his admission, as the state of pulse referred to by Dr. M'Vail had been observed shortly after he came into hospital. He had never met with such an assemblage of symptoms before without also at the *post mortem* finding aneurism. The man's death had occurred from sudden syncope.

Dr. Scott Orr had long ago met with a case of obliteration of the aorta, and referred Dr. Steven to a paper in the *Edinburgh Medical and Surgical Journal* of the day, where the case had been carefully reported, and references to similar cases given by Dr. Craig. The value of this paper was also enhanced by the accurate plates it contained.

Dr. Steven, in reply, said that the patient's death had occurred from syncope, due to a very weakened and fatty heart. Unfortunately, he had not thought at the time of tracing out the anastomosing circulation. He begged to thank Dr. Scott Orr for the reference he had given him in connection with obliteration of the aorta.

Dr. James Finlayson showed a case of EXTENSIVE SUB-MUCOUS ECCHYMOSIS IN THE BLADDER, AND HÆMORRHAGE INTO THE TUBULES OF THE KIDNEYS, OCCURRING WITHIN TWO DAYS, FROM A SINGLE CATHETERISATION IN A CASE OF CHRONIC DISTENSION OF THE BLADDER.

Dr. Finlayson said he thought the case of distended bladder which he now showed to the Society would have considerable interest both for the physicians and surgeons present. The bladder was enormously enlarged and hypertrophied, but the patient, who came into hospital chiefly owing to dyspnœa and swelling of the feet, had no idea that there was anything wrong with his bladder or urinary organs. Such an absence of suspicion on the part of the patient was not very uncommon; but it was probably somewhat rare for the patient to pass as much as 4 or 5 oz. at a time, and to go on passing 40 or 50 oz. daily, with such a degree of retention as existed here. There was, indeed, some frequency of micturition, the patient getting up three or four times during the night to pass water; but such an occurrence is by no means uncommon in those who have reached the patient's age—65—from the presence of simple prostatic disease. Dr. Finlayson referred to a similar case he had seen lately, where the patient had no notion that his urinary organs were disordered, although the bladder was much distended; and, in particular, he referred to a case of diabetes insipidus in a boy (already reported by him in the *Glasgow Medical Journal*), where, in the course of years, the bladder had grown, as it were, to accommodate the child's secretion of urine, and where the bladder was enormously distended. This boy could pass about 40 oz. of urine without emptying the bladder; after a pause for a minute or two, he could again pass other 12 oz., and after another little pause, 6 oz. more, the hypertrophied bladder requiring time and periods of rest in order to contract to any complete extent upon its contents, just as in the case of the uterus during labour pains.

The patient in the present case was a temperate man (an abstainer), 65 years old. He was admitted on Christmas Day to the Western Infirmary, complaining of breathlessness and cough for some months, of œdema of the feet and legs of two months' duration, and a history of frequent micturition could be traced back to spring time; but this last part of the history had almost to be extracted from him by leading questions. An examination of the abdomen showed the existence of a large, hard tumour, which felt so superficial as to suggest some hard mass in the abdominal wall, just below the region of the umbilicus. Careful examination on the following day showed that this hard mass was the fundus of the bladder, and this distended bladder could be traced by palpation and percussion down to the pubes. The question came to be, Ought the diagnosis to be settled by passing the catheter? Dr. Finlayson said that from what he had seen and heard, he believed this was not free from immediate danger. Ten years ago, while working with Dr. Gairdner in the Royal Infirmary, he had reported a case where a patient had evidently had a chronic distension of the bladder, existing at least for months. This was associated with double hydronephrosis. When the bladder was emptied by a catheter of its large quantity of urine of low specific gravity, it refilled itself in two hours repeatedly, and when, after a time, a catheter was introduced and tied in by a well known surgeon, hæmaturia supervened, grave constitutional disturbance occurred, with uræmic symptoms, and death. In that case, the hydronephrosis was extreme on both sides, but the perturbation of catheterism seemed to determine the fatal issue. In another case, of which he had heard from Dr. Forrest, a similar course of events followed. A patient with an abdominal tumour of many months' duration had a catheter passed to determine whether the tumour was a distended bladder, although the history seemed to render this view unlikely. The urine drawn off was clear, but it was bloody when next removed; grave constitutional disturbance supervened, and the patient soon died. In this present case, therefore, Dr. Finlayson felt that the introduction of a catheter was a dangerous remedy, and yet with a bladder extending up to the umbilicus, and with œdema of the lower limbs, it seemed proper to try to get some relief to the bladder. After conference with his surgical colleague, Dr. Patterson, in full discussion of the probable nature of the case, as being one with distension of the ureters and of the pelvis of both kidneys, and very likely with some interstitial nephritis, it was decided, on 29th December, to pass a small sized flexible catheter, and to leave it in (no instruments had ever been introduced into this bladder before), but it was found impossible, owing to the state of the prostate, to get in such instruments, and at last Dr. Patterson had to use a full sized silver catheter, and even then the prostate was slightly tunnelled; about 30 oz. of urine was drawn off; no force was used in trying to empty the bladder; part of the urine was clear, but most of it was largely mixed with blood, resulting from the passing of the instrument. The patient was able to pass his water to some extent the same night; it continued to be highly bloody, and as the bladder next day was not greatly distended no more use of the catheter was made. The patient died on the 31st. The death seemed due to mischief in the respiratory and circulatory organs; no shiverings or fever occurred; and, so far as Dr. Finlayson could judge, the death was not determined by the catheterisation. The lungs presented extreme œdema, and they were in a state of senile emphysema; the heart was much enlarged on both sides, and the œdema of the lung, which seemed the immediate cause of death, probably resulted in part from the state of the heart, and in part from disease of the kidney, which, in addition to distension of the pelvis, presented well marked interstitial nephritis, apparently recent or acute, and without granulations.

In this case the hæmorrhage, which was feared from the passing of the catheter, was clearly due, in great part at least, to the mechanical effect of the instrument, and so no great stress was placed upon its occurrence; but, after death, the surface of the bladder was found to present very numerous spots and patches of sub-mucous ecchymosis, and there was thus a strong confirmation of the idea that the sudden removal of a chronic pressure within the urinary organs might lead to hæmorrhages from the blood-vessels on the surface being thus relieved from the support to which they had been so long accustomed. Very distinct hæmorrhages into the tubules of the kidney were also found in microscopic sections. The urine passed before the introduction of the catheter was of low specific gravity, 1010 or 1011, perfectly clear, free from blood and pus, gave no reaction with guaiac, and presented no blood corpuscles in the sediment. There was, indeed, a small quantity of albumen present, and the microscopic examination showed a few hyaline and granular tube casts. It was clear, therefore, that the sub-mucous hæmorrhages and the bleeding into the tubules of the kidney had occurred *after* the catheterisation, or some traces of blood would have been detected.

The occurrence of bleeding during catheterisation disguised the significance of this case to some extent; but Dr. Finlayson referred to another patient, seen about a year ago, a gentleman over 60, with enlarged prostate, and also with dilatation of the pelvis of the kidneys. This gentleman died of a sarcomatous growth in the abdomen, but, during his illness, acute retention of urine occurred; the catheter was passed by Dr. Hector C. Cameron with perfect ease and without the least blood coming (this was in the course of the forenoon), but when he drew it off again at night, with equal ease, the urine was highly bloody, and this hæmaturia continued for several days. In the sediment in this case there were renal tube casts, indicating very plainly that the hæmorrhage was, at least in part, of renal origin. The case of this gentleman and the case now shown of the sub-mucous hæmorrhage in the bladder and the tubular hæmorrhages in the kidney, found two days after catheterisation, seem to prove that the sudden relief to the pressure within, by withdrawing the urine, may lead to this accident; and if hæmorrhages occur in the kidney, they may readily set up acute inflammatory changes, which, in the victims of chronically diseased kidneys, from long standing obstruction, may be easily supposed to determine perturbations in the renal functions leading up to rapid death.

Mr. Maylard was inclined to think that the hæmorrhage, which occurred from the bladder in the cases given by Dr. Finlayson, was more likely to be due to the injury so frequently inflicted upon the prostate by the catheter, rather than to the cause assigned. In the case in question the clot in the prostatic wound appeared distinctly as if the hæmorrhage had originated there, and not in the ruptured vesical or renal vessels. It was interesting to know whether the hæmorrhage occurred at the time of passing the catheter.

Professor M'Call Anderson said he was reminded of a case of distension of the bladder, reported by the late Dr. Macfarlane, where the abdomen was like that of ascites. After death the condition was found to be due to an hour-glass shape of the organ. The catheter had, during life, only reduced the swelling by a half; and the reason was, that a calculus blocked up the communication between the two divisions of the organ.

Dr. Coats asked if it was not possible to have distinguished in this case whether the bleeding was from the bladder or the kidney.

Professor Buchanan asked if the man was aware of his urinary disturbance, and said that, in his opinion, if the catheter was more used as an aid to diagnosis there would be fewer cases of distension.

Dr. Finlayson replied that the only symptom of urinary trouble was that of increased frequency of micturition, and that had only been complained of since spring last.

Dr. Donald Fraser, Paisley, showed a specimen of ulceration of the duodenum, with thickening of the pylorus. The specimen had been removed a year ago, and illustrated the most extensive duodenal ulceration in the neighbourhood of the pylorus. The pylorus was much thickened and contracted, and the stomach distended. The man, who had long suffered from dyspeptic symptoms, died of syncope, and the stomach, after death, was found filled with grumous fluid. At the *post mortem* the ulcer gave way under the finger, and its base was found to be formed by the gall bladder.

Mr. Maylard showed a specimen of FRACTURED PATELLA of six weeks' standing, illustrating a CAUSE OF WEAK UNION, and read the following notes and remarks concerning it:—

This specimen of fractured patella I had the opportunity of removing from a patient who was under the care of Dr. Cameron in the Western Infirmary, and who, after being six weeks under treatment, died suddenly from aneurism of the aorta.

It illustrates very well one possible cause of weak union, and perhaps offers one argument in favour of treatment by wiring.

Dr. Cameron has kindly favoured me with the clinical features of the case, which I briefly give to show how the fracture was produced. The patient, aged 59, was carrying a weight of over half a hundred weight upon his shoulders, when, happening to put his foot upon a piece of ice, he slipt and fell down, striking his right knee. When admitted into the hospital the fragments were about a quarter of an inch apart, and the joint considerably distended with effusion.

Looking at the patella, it will be seen that the line of fracture is transversely through the centre of the bone, with some irregularity at the inner margin. The interval between the two fragments, when both are bent forcibly forwards, is about half-an-inch, and when bent similarly backwards about an inch and a-half. The anterior margin of each fragment is sharp like the teeth of a fine saw, and these, becoming arrested after some little separation at the time of the accident, have cut through partially the overstretched supra-patellar aponeurosis, which has then sunk in, or rather been sucked in, between the two fragments. Those fibres which are not severed—and they are the deeper—are tightly stretched over the fractured surfaces. On each side of the patella the fibres of the quadriceps extensor are seen intact, so that the injury is limited solely to the bone and its aponeurosis. Still further, it will be noticed that what union has taken place is between the fragments posteriorly, there being little or no attempt at the formation of fibrous tissue in front.

Such is briefly the description of the specimen, and I think it offers a few points of interest. First, although the patient's history would lead one to suppose that the fracture was due to his striking his knee on the ground, the conditions of the parts point unmistakably to the rupture occurring antecedent to the impact. Thus the position of the line of fracture, occurring as it does through the centre of the bone, is where, in a flexed position of the limb, and under a sudden contraction of the quadriceps, the greatest strain is felt; indeed, to express it perhaps more clearly in "mechanical" language, this line would be a fulcrum of a lever of the first order.

That which, however, appears to be the strongest argument in favour of antecedent rupture is the fact of a separation of the fragments occurring before the severance of the aponeurosis This could hardly take place in a direct injury to the bone where both would be more likely to be divided at the same place and at the same time, or the aponeurosis would escape altogether. I should not have alluded to this now usually accepted theory of fracture, except that I thought the specimen a very good practical illustration.

Another interesting point is the amount of union which has taken place after six weeks' treatment. The separation of the fragments, to begin with, was very slight, only about a quarter of an inch, and this narrow interval was no doubt due to the fibres of the aponeurosis on each side of the patella remaining intact, so that the action of the quadriceps upon the upper fragment was considerably impeded. Why, then, with so narrow an interval, was there not uniform ligamentous union? Why also should the posterior margins be closely approximated and firmly united while the anterior gape and show no signs of union.

The want of union in front must be due, I think, to the interposition of the detached piece of aponeurosis, together with those deeper fibres which have escaped severance and are tightly stretched over the fractured surfaces. The gaping of the anterior edges depends, on the other hand, not upon any displacement, the immediate result of the accident, but upon the subsequent effusion into the joint, which, as it becomes more and more distended with fluid, projects forwards and enters the free fractured margin, any marked displacement of the upper and lower borders being prevented by their fixity. The result, therefore, will be—supposing no very great interval exists, as in this case—that the posterior margins are brought into close contact and the anterior are widely separated. Now, as the effusion subsides, the plastic material, which will in all probability have commenced to be thrown out between the closely approximated posterior edges, will have rendered adherent to it the ruptured aponeurosis in front, so that, on the complete subsidence of the effusion, all endeavours by means of treatment to approximate the anterior margins will be effectually prevented. The specimen clearly illustrates what I have just stated, for any attempt to bring together the anterior edges only more completely doubles in the adherent aponeurosis. It thus appears that in these two factors we may find reasons why in some cases of but slight intervals between the fragments we fail to obtain a firm and useful limb. Consider an exaggeration of the condition in this specimen, and should we not then have an amount of ligamentous union insufficient for powerful extension. Again, on purely mechanical grounds, it stands to reason that the less the union in front the less effectual is the leverage action of the patella.

I don't know that I ought to say anything about treatment; still, if my conclusions be correct they are suggestive, and upon the strength of them I shall venture the following few remarks:—

First, I may be pardoned, I hope, for reiterating such a surgical axiom as that each case should be treated on its own merits. To choose any one particular mode of treatment for all cases of fractured patella is a mistake. Thus, to adopt the somewhat conventional method of pulling down the upper fragment and raising the lower one by strapping, would, where there is but a slight interval, tend to arrest the free fractured edges, and so produce one of the evils I have already described. Any very marked retracting action of the quadriceps extensor is prevented by the continuity of the lateral fibres of the aponeurosis, so that all that is really needed is to steady and fix the fragments by an early applied figure-of-eight bandage over the knee joint with a proper position of the limb.

Again, in a similar case of but a slight interval the fluid in the joint should be early evacuated, and any subsequent distension prevented; and lastly, where after treatment for some weeks there still remains a distinctly tangible gap, cutting down, re-freshing the fractured surfaces, and wiring might be reasonably considered. I need hardly say that I have limited myself to the consideration of the treatment of only such cases as we may suppose the present to illustrate.

There is one other feature in the specimen, however, which perhaps affects the question of treatment in all cases of transverse fracture of the patella whatever the width of the interval between the fragments; and that is, the possibility of firm and serviceable union being really prevented by the interposition of a severed piece of aponeurosis or its torn fibres. It seems that here there may be a strong argument in favour of early wiring of the fragments in the way first performed in Glasgow by Dr. Cameron in the Royal Infirmary, and recently so ably advocated by Sir Joseph Lister. I am not in a position to say much upon the advisability or not of adopting such a severe method of treatment, but I merely introduce the consideration to elicit the more matured opinion of my experienced confreres.

Before concluding, I must refer to the ingenious remarks upon this particular subject of union in fractured patella, made by Dr. Macewen in a clinical lecture recently published in the *Lancet*. Dr. Macewen there sought to show that the cause of weak union was due to the ruptured aponeurotic fibres overlapping the severed surfaces. This specimen, to some extent, I think, certainly supports that view. For this piece of the supra-patellar aponeurosis to have been cut off as this has been, considerable separation of the fragments must have first occurred before the severance took place. Hence, it is easy to conceive that with less sharp margins to the bone the membrane might give way in one irregular line, and the stretched and jagged edges fall over, or rather be sucked in, over the fractured surface.

MEETING V.—*21st January, 1884.*

The President, PROF. M'CALL ANDERSON, *in the Chair.*

DR. MACEWEN showed a specimen of a FISH WITH A CURVATURE OF THE SPINE. A great part of the interest in this case centred in the fact that the fish had been under observation for some time before its death, as it had been one of a number kept in a glass vessel at the Children's Hospital. Some months ago it was thought that a slight curvature of the spine could be noticed, but at first there was some doubt. Soon after this the curvature became quite apparent, and this was followed by paralysis of the lower fin of the tail.

In a short time the caudal dorsal fin became similarly affected. It is quite possible that there might be a history of traumatism in this case, because the people in the hospital were in the habit of lifting the fish out of the vessel in a ladle when they changed the water. Dr. Macewen had written to Professor Young about the matter, but had not yet received an answer; and when the specimen had been dissected he would again bring it under the notice of the Society.

Dr. Wm. Gibb Dun showed a specimen of ENCEPHALOCELE, and read the following notes:—

The case is one of occipital encephalocele in a fœtus of the seventh or eighth month. The tumour is fully twice the size of the fœtal head, and the contents consist apparently of the greater part of the brain, the coverings being the cerebral membranes, and the thinned and distended scalp. There is a fringe of hair round the base of the tumour, but none upon the tumour itself, which at birth was of a dark bluish hue. The other parts of the fœtus are quite normal in appearance, although, as a whole, it seems small.

Mr. Jonathan Hutchinson, in the first volume of his *Illustrations of Clinical Surgery*, says—"These cases are met with more frequently at the back of the head than elsewhere. . . . . The occipital bone is usually deficient in the middle line, just above the foramen magnum, and through this opening a pedunculated tumour, often containing part or whole of the cerebellum, escapes." Professor Cleland makes not a bad comparison between the appearances of a case, very similar to the present one, and the fashionable "chignon' of former days. In the case which he dissected, an account of which appeared in the April number for last year of the *Journal of Anatomy and Physiology*, the condition seems to have been produced by a dropsy of the olfactory bulbs and infundibulum.

The history of the present case is as follows:—Mrs. P. was supposed to be in the eighth month of her third pregnancy when I was called to attend her. She complained of severe and constant pain in the right hypochondrium, was unable to sleep or to take food, and micturition was frequent and painful. The abdomen was enormously distended and protuberant, the parts of the fœtus could not be distinguished, and the sounds of the fœtal heart, though carefully sought for, could not be heard. There was distinct fluctuation at the upper part of the abdomen, a circumstance which caused me to think that probably there was an unusually large secretion of liquor amnii. On vaginal examination the os uteri was reached with difficulty, and was found to be slightly patent, but the presentation could not be made out. As the rectum was loaded an enema was ordered, and friction with warm oil advised over the right hypochondrium. The patient seemed to have received some ease from this treatment, and I did not see her for three days again. She was then in greater pain than ever, and being still unable to make out the presenting part, I determined that if by the evening there was no material change I would administer chloroform, and introduce my hand into the vagina so as to satisfy myself exactly regarding the presentation. In the evening I accordingly found it necessary to do so. The os had now opened somewhat, and was easily dilated so as to admit the hand. Nothing was to be felt but the bag of membranes. While endeavouring to make out some part of the fœtus the membranes ruptured, and immediately there escaped an enormous quantity of liquor amnii, drenching the bed and flowing over on to the floor, where a vessel was placed to receive it. The quantity must have amounted to a good many pints, and was quite beyond any previous experience of mine. I then seized the fœtus by the feet and delivery took place easily, although I was rather afraid that from the rapid manner in which the os had been dilated it might contract as quickly and so delay delivery. The placenta was adherent to the wall of the uterus low down, but was easily removed. The uterus contracted firmly, and the woman, who was pretty deeply under the influence of chloroform, soon regained consciousness. She made a quick and good recovery.

I am not certain whether the fœtus was alive when born. Movement, however, was felt up to within a short period of delivery. Early in her pregnancy the woman had received a blow on the head. A swelling had formed here, and as was to be expected, there seemed to be an inclination, on the part of her friends, to associate this with the condition of the child. Strange tales are told regarding "maternal impressions." I merely mention the circumstance as it was told to me.

The practical point which the case illustrates is, I think, the diagnosis and treatment of dropsy of the amnion. The cause of this condition is apparently obscure, but is believed to be connected with a diseased condition of the placenta or membranes, or of the fœtus itself. In the present instance I could make out nothing abnormal in the appearance of the placenta, except that it seemed thinner than usual, but this I ascribed to the tearing it had received in the process of separation. As regards the encephalocele, this, I fancy, is chiefly interesting to the anatomist, and I have accordingly handed the specimen to Professor Cleland, who may, perhaps, at some future meeting of this Society, exhibit the results of the dissection.

Dr. Macewen showed a patient whose BRAIN HAD BEEN OPERATED ON FOR THE RELIEF OF LEFT HEMIPLEGIA, and who has recovered the power of movement to such an extent as to enable her to walk freely about, though with a paraplegic gait; to raise her arm to the level of her shoulder, and to grasp with considerable power, though there is a deficiency in co-ordination of movement of the hand.

She had a syphilitic history. The hemiplegia was preceded by a tingling sensation and a numbness of the left arm and leg, which increased until it ended (within six weeks from its commencement) in complete motor paralysis and a deficiency in the perception of touch. The left side of the face was also slightly affected. This was accompanied by mental confusion and loss of memory.

Full anti-syphilitic treatment had been tried along with counter-irritation to the head, previous to her coming into the Royal Infirmary, and while she was in the medical wards of that institution. These did not seem to have relieved in the slightest the condition spoken of above.

Trephining was performed over the middle of the ascending frontal and parietal convolutions. Internal table of the disc removed was found softened and thicker than usual, having on its internal surface a series of projections or roughnesses, some protruding for nearly $\frac{1}{8}$ inch beyond the general level. A second opening was made over the occipital region, and a similar thickening of the internal table was found. Opposite to the first opening the dura mater was paler than normal, and somewhat thickened. It was elevated, and a false membrane of yellow colour, and about $\frac{1}{16}$ of an inch in thickness was removed. An incision was made into the brain in the direction of the paracentral lobule, when a gush of grumous red coloured fluid escaped out of the opening. Its quantity was not measured, as it could not have been collected. Approximately, there would be about a couple of drachms. The brain pulsations previously were not discernible, but, after the escape of this collection of fluid, it was thought that very feeble pulsations were seen. Some of the surgeons standing by doubted the presence of the cerebral pulsation.

The discs of bone were carefully divided into segments and replanted, and are now quite firm. Wounds treated without pus production.

The day after the operation she expressed herself as very much better. On the third day she moved her toes. Within a week she lifted her leg from the bed and stated that she was so much better that she could turn in bed, and believed she could walk. The fingers were moved within a week. Her mind greatly changed for the better, her memory improving and her intelligence becoming much brighter. She can now walk freely about, and does a considerable amount of domestic duty in the ward. She lifts her arm to the level of her shoulders, and can grasp with considerable force. (Patient shown).

Dr. Finlayson said that he had lately had the opportunity of seeing this case, at Dr. Macewen's request, in private, and he thought that there could be no possible doubt of the improvement, which had taken place, being due to the operation. It is known that in cases such as this remarkable improvements do take place as the result of purely medical treatment; but in this case it is certain that the improvement was due to the operation. This method was quite a new one, and depended greatly upon the advances that had in recent years been made in cerebral localisation. It was quite different, too, in its nature from operations on brains injured by violence. Although the case shown to-night was a very successful one, yet advance in such a procedure could scarcely be expected without a certain number of misfortunes.

Dr. Robertson thought that the improvement in this case was to be attributed to the surgical treatment. The result of prolonged anti-syphilitic treatment was negative, and thus the marked improvement might justly be attributed to the operation. Further, by the operation great relief of pressure was obtained, grumous fluid was evacuated, and a false membrane removed. Each of these conditions was capable of causing a hemiplegia, and he had no difficulty in understanding how their removal or relief should benefit it. He was also interested in hearing Dr. Macewen record the assistance in diagnosis that he had obtained from percussion of the skull in this case. Dr. Robertson then referred to a *post mortem* he had that day on a case of leptomeningitis, which it was possible might have been relieved by some such procedure.

Mr. H. E. Clark thought that the interest of these cases was growing, and that the time was coming when we would need to revise our ideas of trephining, and there was no doubt that many cases might be improved if we knew exactly where the lesion was situated. He thought that sometimes we were apt to fall into error by following too closely the external manifestations of injury, and not paying sufficient attention to the nervous symptoms. He had lately had a case of head injury, which first rallied, and then a day or two after sunk into stupor. There was paralysis of the right arm and leg. He took as his indication a puffy swelling on the right side, exposed the bone, found no fracture, and trephined. The patient died, and at the *post mortem* a large clot was found covering the left hemisphere of the brain. It would have been better for him in this case to have taken as an indication the distribution of the paralysis. Mr. Clark also referred to a case of epilepsy, where he had trephined and replaced the bone.

Dr. Newman thought that syphilitic cases were unfavourable for operation, because syphilitic lesions were multiple, and were very apt to spread. In this case it had been shown that there were quite a large number of prominences on the interior of the skull. He thought it was a decided advantage to divide the disc of bone into parts, and he was of opinion that it would be a still greater if the centre of the disc were perforated before dividing it, so as to admit of the insertion of a small drainage tube. Granulation tissue was apt to block up the spaces between the particles of bone and prevent free drainage.

Dr. J. Lindsay Steven was particularly interested in the description of the condition of the bone which had been given by Dr. Macewen, as it recalled very vividly to his mind a case of syphilitic disease of the interior of the skull, which he had some time ago seen in the *post mortem* room of the Western Infirmary. In this case the dura mater was extensively separated from the bone by a thin layer of pus and soft gelatinous or granular like tissue, and the bone generally had a worm eaten like appearance. He could not recollect anything regarding either the clinical or further pathological history of the case; but the appearances of the interior of the skull were such as not to be easily forgotten. In the light of what Dr. Macewen had said Dr. Steven could not help being of opinion that this was a case which might have been considerably benefited by such an operation, if for nothing else than simply as a means of establishing drainage.

Dr. Thomson some time ago had seen a case resembling in a good many points that presented by Dr. Macewen, and in which there was a distinct syphilitic history. In this instance anti-syphilitic treatment restored the man to almost the complete use of his limbs, and he thought it would be a good thing in Dr. Macewen's case to commence a course of anti-syphilitics to prevent any recurrence of the complaint.

Mr. Maylard was anxious to know if there was any association to be made out between the lesion in the bone and *dura mater* and the grumous fluid which was evacuated from the brain. To him they seemed to be two independent syphilitic lesions.

Dr. Macewen, in reply, said, with regard to Mr Maylard's question, that the irritation of the bone and the false membrane of the *dura mater* had to do with the paralysis of the arm; and the grumous fluid may possibly have arisen from a clot of blood in the paracentral lobule. Dr. Steven thought that there was a similarity between the case he mentioned and the one shown to the Society to-night; but in the latter case there was no worm eaten appearance, but simply projections from the inner table of the skull. He (Dr. Macewen) had, however, operated on cases where there was this worm eaten appearance. He agreed with Dr. Thomson that it might be right to administer anti-syphilitics, but he had hitherto withheld them in order accurately to judge of the amount of improvement that might justly be attributed to the operation. He thought that multiple trephining was good. On two occasions he had introduced the disc of bone entire: one did well; a small portion of the other necrosed, and the whole had in consequence to be elevated. The advantage of having the bone in small pieces was that you could, under these circumstances, easily remove the affected portion and leave the remainder.

The members were unanimous in congratulating Dr. Macewen upon the success of his case.

Mr. H. E. Clark showed a large malignant tumour of the shoulder, which was removed by operation, and of which the following are the notes extracted from the Ward Journal:—

The patient was a man aged 60 years, who had received a bad strain of the shoulder in a quarrel about seven months ago. A month after this swelling began at the outer end of the clavicle, and gradually progressed until, on admission, it had assumed the following dimensions. Before describing the tumour it should be mentioned that, previous to his admission, it had been incised by a medical man, when nothing but blood escaped:—There was a tumour over the outer part of the clavicle, tolerably spherical in appearance, with a broadish base, which measured about 5 inches by 4. Over the anterior part there was a scab in the place where the incision was made a fortnight ago. Over the most prominent part of the tumour—viz., on the anterior and inner side, there was a bulging like an abscess coming to a head, and at the back of the tumour another similar point not so marked. The skin over the tumour was of a dusky red, and purplish over the protruding points. On palpating the tumour it was found to have an expansile pulsation all over it, most marked at the protruding points mentioned, where also it seemed to be fluctuant. The tumour extended backwards to the spine of the scapula, stopping short of it at the inner and middle parts, but seeming to fuse with it to some extent towards the acromial process, not, however, distinctly spreading over this. It extended outwards to the acromion, seeming to fuse with its inner border as it curved forwards. Forwards and downwards it was at the outer side situated at the level of the third rib, while at its inner part it was no lower than the first rib. Its inner margin was slightly internal to the middle of the clavicle, which seemed pushed forwards and upwards by it, and which followed from within outwards became lost in the substance of the tumour. The skin over the tumour was very tense, and the tumour itself was hardly movable. The arm moved pretty freely at the shoulder joint. Probably owing to the displacement upwards of the clavicle it was found impossible to compress the subclavian sufficiently to control the circulation at the wrist or in the tumour. The patient was in fair health, and there was no marked cachexia. His arteries were fairly healthy. On auscultation a faint systolic beat could be heard in the tumour. The pulse at the wrist on the right side was unaffected. There had never been any swelling of the arm.

The operation for the removal of the tumour was performed 12th December, 1883. Ether was given. An incision was made over the inner end of the clavicle from the inner margin of the tumour to the sterno-clavicular articulation. The clavicle was sawn through at the outer part of the incision and disarticulated at the joint. This piece of the clavicle was removed by separating it from the periosteum, and this was done without much hæmorrhage. The subclavian artery was then sought for in its first part, while doing so the transversalis colli artery was exposed and ligatured. The subclavian artery was found somewhat higher than usual, and was ligatured. During this part of the operation the internal jugular was wounded in retracting the inner margin of the wound, the bleeding point in it was secured and ligatured.

An incision was then carried round the base of the tumour behind the scapula and joined by a similar incision in front. The tumour was next separated from the subjacent tissues, this being easily accomplished, and some arteries proceeding to it were ligatured. The tumour was found not to implicate the scapula, except its acromial process, which was removed. The outer end of the clavicle was removed with the bulk of the tumour. The coracoid process seeming also to be slightly involved was sawn off later on in the operation. The arm was next amputated at the shoulder joint, the incision extending nearly half way down the upper arm so as to obtain a large posterior flap, which was carried upwards and forwards to cover the large area exposed by the removal of the tumour. The edges of the wound were brought together without much tension, after removing a few portions of doubtful tissue. The whole operation lasted nearly two hours. The amount of blood lost was not great considering the size of the wound. The patient kept well and had a fair pulse all through the operation. For some hours after the operation the patient kept well, the pulse being very fair. He soon complained of considerable pain in the shoulder, and $\frac{1}{4}$ gr. of morphia was given subcutaneously, and repeated about every four hours. His condition was satisfactory until about twelve hours after the operation, when he began to get worse; the pain was very severe and the pulse getting feebler, but still fair at that time. He did not improve through the night, only getting a little sleep in spite of his morphia injections. In the morning, as there was a considerable amount of oozing, the dressings were removed and ice applied to the wound. There was not much hæmorrhage, and the ice seemed to check what there had been. He, however, continued to sink, and died about 11 A.M., 13th Dec., twenty-four hours after the operation.

The tumour was examined by Dr. Newman, who found it to be an example of a small round-celled sarcoma, in which there were numerous thin walled blood-vessels.

Dr. Newman referred to the great possibility there was, in the early stages of such cases, of mistaking them for abscesses; and referred to a case in which he had recently performed a *post mortem*, where a tumour of this kind presented a most striking similarity to an abscess.

Dr. Macewen thought that in the present case there could have been little chance of mistaking the condition for abscess.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Deodorisation of Iodoform.—Fournmont (*France Méd.*, 1883, II, No. 19) gives the following recipes for powders containing iodoform, but not of offensive odour:—

(1.)	Acid. carbol. cryst, . . .	1 part.	
	Iodoform, ...	10 parts.	Misce bene.
(2.)	Camphoræ, ...	5 „	
	Carbon. pulv., ...	10 „	
	Iodoform, ...	15 „	Misce.
(3.)	Camphoræ, ...	5 „	
	Ol. menth., ...	2 „	
	Iodoform, ...	15 „	Misce.

—(*Centralbl. f. Chir.*, 1883. No. 40).

"Attar of roses, one minim to the drachm of iodoform, will, on the principle of the survival of the fittest, it is said, get the better of the odour of the latter drug, and the patient, instead of a social pariah, may become a garden of delights unto himself and all his neighbours."—*Boston Med. and Surg. Journal.* Vol. CIX. No. 21.—D. M'P.

A New Method of Propagating the Vaccine Virus.—At a meeting of the Medical Society of Finland on the 10th of November, Dr. Qvist made a communication concerning a new method of propagating the peculiar bacteria upon whose vitality in vaccine virus the efficacy of its contagion depends. It is not necessary to describe this form of bacterium, since it is well known to microscopists. To propagate these bacteria outside of the animal body two conditions are essential:—

(1.) An exposure to common atmospheric air, upon the carbonic acid of which these microzoa live.

(2.) A suitable culture material.

The description of an apparatus which embodies these conditions is not complicated; but unless there is present carbonic acid gas, no development of the vaccine organism can take place. The development of these organisms diminishes more readily in capillary tubes which are quite full of vaccine lymph, thus allowing no air space, and especially if these tubes are hermetically sealed; while on the contrary, tubes whose calibre is of a certain size, and which are open, favour their development. Dr. Qvist's experiments proved to him conclusively that these organisms pass through a decided development in open tubes partially filled with vaccine lymph, but that it is not of the same character, in most instances, with that which occurs in the animal body.

A vaccine bacterium is a veritable *aërobium.* In the lymph spaces of the skin there is undoubtedly a sufficient quantity of air containing carbonic acid gas, but this condition can be more readily controlled outside of the body. It is only necessary to have a porous substance, like the meshes of a common sponge, which shall have the property of imbibing a liquid, whose character will form a convenient vehicle for the cultivation of the vaccine bacteria, but a watch glass answers this purpose even better, because the liquid vehicle placed in it may have its upper surface more thoroughly exposed to the active influence of common air.

Dr. Qvist has used for this culture-fluid egg albumen, which is better than the more solid forms of albumen. Egg albumen has properties which are in many respects similar to the serum of the blood. Yet it is not improbable that the mineral portions of egg albumen combined with ammonia and some organic oxide may serve equally well for a propagating medium, though this remains for further investigation. He prefers to use glycerine with egg albumen, not because it forms an important ingredient of the culture fluid, but because without its presence the albumen is liable to a partial desiccation which prevents the cultivation or propagation. Vaccine bacteria require during their development a larger proportion of glycerine than other micro-organisms, especially those of the fungus character, such as may grow upon old leather, for the following reasons: mould fungi may possibly destroy or prevent the growth of vaccine bacteria; hence, as the former require less glycerine, and are really destroyed by too much of it, the latter may exist more favourably in a vehicle containing rather more glycerine than is ordinarily used in the cultivation of these mould fungi. Therefore this condition of their vitality is an important matter. Mould fungi also live by preference in a liquid vehicle which has an acid reaction; while, per contra, vaccine bacteria like their congeners exist by preference in a vehicle of an alkaline reaction.

The following list of culture fluids used by Dr. Qvist does not presuppose that there are not others possessed of equal value for the propagation of vaccine bacteria, and he recommends, as those which experience has shown him to fulfil all necessary conditions, the first four named :—

No. 1.	Bovine blood serum	1 part.
	Glycerine	1 part.
	Distilled water	1 part.
	Calcic carbonate	1-800 part.
No. 2.	Bovine blood serum	2 parts.
	Glycerine	1 part.
	Distilled water	2 parts.
	Calcic carbonate	1-400 part.
No. 3.	Egg albumen	1 part.
	Glycerine	1 part.
	Decoct. althææ radicis	1 part.
	Calcic carbonate	1-150 part.
No. 4.	Egg albumen	1 part.
	Mucilag. gum acaciæ	6 parts.
	Calcic carbonate	1-60 part.
No. 5.	Tartrate of ammonia	1 part.
	Phosphate of calcium	1-10 part.
	Carbonate of calcium	1-5 part.
	Sulphate of magnesium	1-50 part.
	Chloride of calcium (vel calcium chloretum)	1-100 part.
	Distilled water	100 parts.
	Glycerine	30 parts.

The following means should be used to destroy the organisms which are peculiar to the culture fluid itself. It should be subjected to a constant temperature of 60° C. for one and a half hours continuously on three successive days. Dr. Qvist has proved this means of sterilising the culture-fluid by frequent microscopical examinations of it after the above process, and has not found any organisms develop even in a specimen kept for three months. All the other ingredients used in the culture-fluid have either been subjected to a similar sterilising process or have been used immediately after being warmed or heated.

As he found that the usual summer heat or that of a warmed house in winter, at 19° C. to 20° C., favoured the propagation of these bacteria, he has not experimented with any other variations of temperature.

As seed for transplantation of vaccine bacteria he has used vaccine lymph just ripe or a little old, and which has been found by experimentation to be capable of producing a true vaccine pustule. Then a piece of dry sponge, completely sterilised by the above process, is allowed to imbibe a little of this selected vaccine lymph, and is immediately transferred to a culture-fluid placed in a watch glass.

But a more convenient and safe method is followed by taking a piece of the epidermis from a true vaccine pustule, but taking pains not to allow any of the blood serum to be attached to it, because the epidermis itself is sufficiently saturated with pure vaccine lymph. To do this successfully a small portion of the epidermis should be taken from a vaccine vesicle on the eighth to the tenth day according to the character of its development, by gently lifting and separating it from the whole vesicle, then cleaning it by washing, and then after one or two days' careful drying it should be immediately placed in the culture-fluid. This solid mass of seed, if not injured by outward circumstances, has always proved efficacious.

The watch glass should be three or four centimetres in diameter, and is the most convenient vessel for use when it is desired to propagate rapidly the vaccine virus in a dilute form. The watch glass should be placed on a table or shelf and exposed to moist air and covered by a cleaned tumbler.

When it is desired to produce a concentrated form of virus a test tube is preferable, and it should be corked with a paraffin coated cork, taking care to sterilise the paraffin, and to allow a small air-hole on the side of the cork.

If the surface of the culture-fluid be examined, even in a day or two, it will become turbid, and in the course of a week or ten days scales will form over its upper surface in a series of floating islets, and a fine powder will fall down to the bottom of the glass. This powder, which is also attached to the scales, contains the bacteria. At first the culture-fluid is turbid, but gradually it assumes a yellow or straw colour, and these physical appearances are increased by obliquely reflected light.

Vaccine lymph thus artificially prepared can be preserved in capillary glass tubes and used as required.—*Boston Medical and Surgical Journal.* 20th December, 1883.

On the Etiology of Tabes Dorsalis.—By Prof. Erb Heidelberg (*Berl. Klin. Wochenschr.*, 32, 1883).—To his first hundred cases of locomotor ataxia, of which, as is known, 88 were previously infected with chancre—*i. e.*, syphilis, Prof. Erb has added a second, with the following results:—

Cases without any preceding syphilitic infection, .	9 per cent.
Cases with preceding syphilitic infection, . .	91 per cent.
(Of the latter, with undoubted *secondary syphilis*, .	62 per cent.
And with *chancre without marked secondary symptoms*,	29 per cent.)

Five of the latter were originally reckoned as *hard* chancres, whilst 10 had been treated with mercury, iodide of potassium, &c., so that a good number of these even might be considered as syphilis.

The following table shows the time intervening between the onset of the tabes and the syphilitic infection:—

Between	1	and	5	years,	13 cases.
,,	6	,,	10	,,	31 ,,
,,	11	,,	15	,,	25 ,,
,,	16	,,	20	,,	15 ,,
,,	21	,,	25	,,	5 ,,
,,	26	,,	30	,,	1 case.
	Unknown,				1 ,,

The author instituted a test experiment, in which he carefully inquired at all his male patients, over 25 years of age (1,200 cases) who were not suffering from tabes or from syphilis at the time, as to a preceding venereal infection, and he found—

Non-infected,	77·25 per cent.
Previously infected,	22·75 per cent.

(Of the latter 10·25 per cent with secondary syphilis, 12·5 per cent with chancre alone).

Supposing that one does not consider the cases of chancre, but only those of confirmed syphilis, then among ataxics 62 per cent and among non-ataxics only 10 per cent have previously suffered from syphilis.

A table of all the predisposing causes (syphilis, cold, hardships, excesses, wounds), shows that syphilis is by far the most abundant; and the others generally go hand in hand with syphilis, very seldom alone.

Concerning the objection that pathological anatomy testifies against the syphilitic origin of syphilis, Professor Erb considers that histological researches are not sufficient to determine what is and what is not syphilitic in origin.

Erb holds, therefore, *that syphilis is one of the most important, if not the most important condition, concerned in the causation of tabes.*

In conclusion, the author communicates the following case, which is of interest, not only as giving an insight into the nature of tabes, but also as showing the possibility of a later development of syphilis:—In a man, who only had chancre and bubo, the recognised initial symptoms of tabes showed themselves 8 years later, which (15 to 20 years after infection) developed into the typical disease. After this happened, and for the first time (20 years after infection) syphilitic manifestations made their appearance in the skin, joints, and testicles. —*Deutsche Medizinal Zeitung.* Oct. 1883. No. 40, p. 568.—J. L. S.

A Case of Supposed Dislocation of the Tendon of the Long Head of the Biceps Muscle.—In *The American Journal of the Medical Sciences* for January, 1884, Dr. J. William White records a case of this very rare form of luxation, and reviews the history of the few other cases in which this accident is supposed to have occurred. He finds that the recorded evidence of the occurrence of dislocation of the tendon of the long head of the biceps muscle may be divided into two general classes:—1. The reports of clinical cases in which certain symptoms were referred by the writers to this displacement, but in which its existence was not otherwise confirmed. 2. The reports of cases in which the tendon of the biceps was found luxated at an autopsy, or during a dissection, but in many of which no clinical history was obtainable. The study of the literature of the cases recorded leads to the conclusion that although for more than a hundred years cases of supposed luxation of the tendon of the long head of the biceps muscle have been reported or alluded to by surgical writers, yet they have been so poorly observed or so carelessly described, that they fail altogether to carry conviction, the one case (Soden's) which possesses any strong element of probability being itself open to reasonable doubt.

The symptoms in Dr. White's own case, which led him to the conviction that there had been true traumatic luxation of the bicipital tendon, may be enumerated as follows:—

1. The recognition of the bicipital groove, empty, which, if its existence be admitted, is pathognomonic. 2. Recognition of the tendon itself. 3. The inward rotation of the arm. 4. A slight depression under the tip of the acromion, a prominence of the shoulder in front, and a flattening behind. 5. Diminution in the vertical circumference of the shoulder. 6. Shortening of the arm as measured from the tip of the acromion to the external condyle. 7. Elevation of shoulder, tilting up of acromion, and elongation and narrowing of axilla when the arm was carried upwards. 8. The peculiar depression situated over the bicipital groove. 9. The line of ecchymosis following and strictly limited to the course of the biceps muscle. 10. A creak or "squeak," heard distinctly on carrying the elbow away from the side. 11. Flexion of the forearm on the arm was painful, the pain being sharp, lancinating,

and felt at the front of the shoulder; flexion during supination was much more painful than flexion during pronation. 12. When extension of the forearm was attempted a tense line along the edge of the biceps could be both felt and seen. 13. The pain felt over the joint was also felt along the line of the biceps as far as its insertion, and the patient still has a "drawing" sensation over that region. 14. The arm was preternaturally mobile for some time after the accident. 15. The position of the patient after the accident. 16. The character of the force producing the difficulty.

The rationale of these symptoms is very fully explained.

Chromic Acid as a Caustic.—Dr. Squibb says chromic acid is a valuable caustic, "because it is self limiting in its action in a degree that no other destructive caustic is. It is an active oxidising agent and destroys the tissues to which it is applied by oxidation. In this respect it is like other caustics, as nitric acid. But every molecule of chromic acid which destroys a molecule of organic tissue is itself destroyed and rendered inert by being reduced to an insoluble oxide of chromium; and this principle and degree of self limitation is not obtained from any other caustic.—*Canada Lancet.* Oct. 1883.—J. L. S.

Peripheral Nervo-Tabes, or Locomotor Ataxia from Peripheral Neuritis with absolute integrity of the Posterior Roots and of the Spinal Cord.—M. Dejérine records two cases under the above heading. The first, a man of 40, entered the Hôpital Lariboisière for weakness of the lower limbs. There was want of co-ordination of the movements, absence of the patellar tendon-reflex, anæsthesia and analgesia in the legs; no myosis. The patient died, and the spinal cord and the spinal ganglia were found to be absolutely healthy, as also the posterior roots; while the cutaneous nerves, taken from different parts of the legs and thighs, and treated with osmic acid and picrocarmine, showed very pronounced parenchymatous neuritis. The second case was at the Hôtel-Dieu, and had regard to a female, 50 years of age. For several months she had felt pains in the legs, and shortly after she began to have difficulty in walking, until at last she became quite unable to walk. For several weeks she had suffered from pains in the arms. She was slightly cachectic, and had probably been addicted to alcohol. She was still able to maintain the upright posture, but only if her eyes were open. No myosis. Emaciation of the muscles of the body.

Muscular force still well marked. Slight diminution of Faradic contractility. Absolute inco-ordination of the lower limbs, less intense of the upper limbs. Anæsthesia and analgesia very marked, with retardation for several seconds in the transmission of painful impressions, over the whole of the body, the face excepted. No thermo-anæsthesia. Absence of patellar reflex. The disorders of sensation much more marked the more distal the point of limb examined. At the necropsy there was found interstitial hepatitis. The spinal cord and the spinal ganglia were absolutely healthy, but the cutaneous nerves of the legs, thighs, arms, abdomen and thorax presented an extreme degree of parenchymatous neuritis. There was not a healthy tube for preparation in the nerves of the skin of the thighs and legs; osmic acid had no effect on them. Similar changes in the skin in other regions, diminishing somewhat in intensity towards the superior extremity of the body. Slight changes in the intra-muscular nerves; slight multiplication of the nuclei of the primary bundles. Absolute integrity of the posterior and anterior roots in the whole length of the cord.—*La France Médicale.* 30th October, 1883.—G. S. M.

Calcium Sulphide in Aural Diseases.—Bacon (*Archives of Otology,* xii, 2,) has used calcium sulphide in many cases of acute otitis media with great advantage. In several cases where the membrana tympani was highly congested and bulging, all the inflammation subsided under the use of this remedy, and he believes that it will arrest the progress of many cases if given sufficiently early in the course of the disease. Its most decided action seems to be in those cases of otitis media in which the discharge has already commenced, as well as in cases of furuncles in the external auditory canal, where it will either arrest the inflammation and cause the boil to dry up, or it will promote suppuration and cut short the disease. The pain so frequent in these diseases, even when the periosteum is involved, is often relieved at once. In diffuse inflammation of the external auditory canal, and in mastoid disease, whether affecting the pneumatic cells or the periosteum and tissues externally, great benefit, according to Bacon, will be obtained from its use.—J. A. A.

The Prevention of the Disorders of Hearing and of Vision caused by large doses of Salicylate of Soda and of Sulphate of Quinine.—According to M. Schilling, the hyperæmia, which is the cause of these disorders, may be prevented by combining these remedies with ergot of rye; thus:—

1. ℞.

Ergot of rye, ...	10 grms.
Make an infusion of	180 grms.
Add	
Salicylate of soda, ...	10 grms.
Brandy, ...	40 grms.

Sig.—℥ss every hour.

2. ℞.

Ergotine, ...	1 grm.
Sodæ salicyl., ...	10 grms.
Aq., ...	250 grms.

M.

Sig.—℥ss every hour.

3. ℞.

Ergot of rye, ...	1 grm. 50.
Quin. sulph., ...	1 grm.

M.

Sig.—To be taken in the dough of bread.

4. ℞.

Ergotine, ...	1 grm.
Quin. sulph., ...	1 grm.

M.

Sig.—To be taken in the dough of bread.

—*La France Médicale.* 8th Sept., 1883.—G. S. M.

Hæmaturia after the use of Salicylic Acid. Dr. M. Loeb (*Cbl. f. Klin. Med.*, No. 37, 1883).—The author, in a patient suffering from acute articular rheumatism, saw hæmaturia, with abundant albumen and tube casts, set in 24 hours after the use of 15 grm. of salicylic acid, disappearing after stoppage of the drug. Gerhardt about the same time mentioned to the writer that he had observed blood from the kidney during the use of salicylic acid.

The author, along with others, has also observed albumen occurring in the urine after comparatively small doses of salicylate of soda.—*Deutsche Medizinal Zeitung*, No. 40. 4th October, 1883.—J. L. S.

Paget's Disease of the Nipple, or Malignant Papillary Dermatitis.—Dr. S. Sherwell, in the January number of *The American Journal of the Medical Sciences,* records two cases of this rare affection, the diagnostic evidences of which he summarises as follows:—

1. The subjective symptoms, itching, burning, &c., are those of an eczema, and not those of an ordinary carcinomatous affection, but they are more marked than in an ordinary case of eczema.

2. The objective appearances are like eczema; the discharge is absolutely similar to that of eczema; it stiffens linen, and forms crusts entirely undistinguishable from those of an impetiginous eczema. The colour of the surface is, perhaps, occasionally more livid, but the border is not more sharply defined than is common in that trouble; the somewhat elevated appearance of the patch simulates exactly the acutely macerated and swollen conditions of the lower epithelial layers frequently found in eczemas.

3. The disappearance of the nipple, which is spoken of by Mr. Henry Morris as a "melting away."

4. The retraction of the nipple or tissues immediately beneath, if retraction there be, is not to be distinguished as such, as is so easy in ordinary cancer.

5. The "malignant papillary" feature, as described by Thin, is a very diagnostic point, and would of itself instantly resolve any doubts as between Paget's disease and a true eczema.

6. The extreme length of time before the appearance of anything like positive evidence of carcinoma.

Observations on the Epidermic Scales of Scarlet Fever during the period of Desquamation. By Dr. Pohl Pincus, of Berlin. (*Cbl. f. d. Med.*, Wiss. No. 36, 1883.) —The epidermic fragments on their inner surface were covered with an alcoholic solution of methyl violet, then laid in distilled water, then on the microscopic slide, dried over a spirit flame, and mounted in oil of cloves and Canada balsam. In some parts of the mass are found lying beside one another in small numbers violet-coloured micrococci of about $\frac{1}{2}$ mm. in diameter, which are separated from their position inside the cells. The author does not believe that these organisms, found in epidermic masses on very different parts of the body, are the result of dirt or contamination, but thinks it much more likely *that they are the cause of the scarlatina.* Such observations show also the great necessity for disinfecting the scales.

The author believes too that the scarlatinal inflammation begins in the upper layers of the skin and spreads to the deeper, and that shortly after the poison has effected a lodgment in the rete the fever manifests itself.—*Deutsche Medizinal Zeitung*, No. 40. 4th October, 1883.—J. L. S.

The Localisation of Motor Areas in the Brain.—In the *Revue de Médecine* for May, MM. Charcot and Pitres have commenced a further study of the motor areas in the cortex of the brain, by analysing all recorded cases that bear on the question. In the present article they deal solely with the non-motor region, this comprising the sphenoidal and occipital lobes, the superior parietal lobule, the lobule of the *pli courbe*, the island of Reil, the cuneus and præcuneus, the orbital lobule, and the anterior part of the first, second, and third frontal convolutions.

Since their paper on this subject in 1878, forty-four cases have been recorded of damage to these areas either by softening, hæmorrhage, compression, or irritation, no matter how caused, without any permanent disturbance of motor function being produced. The only point on which any doubt could be permitted is as to whether the lobule of the *pli courbe* is concerned in raising of the upper lid. MM. Charcot and Pitres think not, and they emphasise the fact that lesion of this area is *sometimes, but not always*, followed by ptosis. They conclude that when a lesion of the brain does not involve, either by compression or irritation or directly, the ascending frontal or parietal convolutions or the paracentral lobule, it will not give rise to any disturbance of movement.—*Birmingham Medical Review.* Sept., 1883.—J. A. A.

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MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY.

Session 1883-4. Meeting III.—*7th December, 1883.*

Dr. Gairdner, *President, in the Chair.*

THE CHOICE OF OPERATION IN INTESTINAL OBSTRUCTION.

By Prof. G. H. B. MACLEOD.

I desire, Gentlemen, to bring before you this evening, in as succinct a form as I can, an important question of practice—viz., What operation is best to perform in cases of intestinal obstruction? I had promised Dr. Gairdner, your president, to open the session with a paper upon this subject, but I was unable to fulfil my intention from want of time. Even now, what I have to say has had to be put together at express speed.

I need not dwell on the serious and anxious nature of cases of intestinal obstruction, nor the great difficulties which beset the question of operation for its relief. Those who have had to do with such affections will best appreciate the difficulties and anxieties which surround them. That obstruction, from some cause or other, is very common in a large community like this will be conceded; and so, without further introduction, I may at once pass on to the special point I wish to submit to your opinion and criticism.

I had the honour a few years ago to read a paper before this Society on the difficulty of diagnosing the various causes which may give rise to obstruction of the bowel. While that question is to-night outside my thesis, yet I must briefly recall the various conditions which may cause obstruction, as otherwise a lucid statement on the surgical operations which may have to be performed cannot be made.

It is hardly necessary for me to say that all forms of external hernia, as well as vices of conformation (as, for example, imperforate anus), hysteria (which occasionally mimics the symptoms of obstruction), are not now taken into account. What, then, are the causes of obstruction which are met with in practice?

(A) *Causes situated in the wall of the bowel itself:—*

(1.) Spasm, as in lead colic or enteritis.
(2.) Stricture, the result of cicatrisation, ulceration, or malignant disease.
(3.) Volvulus, twists of the bowel.
(4.) Internal hernia, whether diaphragmatic, obturator, or that which takes place through the foramen of Winslow.
(5.) Intussusception, or invagination.
(6.) Paralysis of the bowel.

(B) *Causes lying external to the bowel:—*

(1.) Adhesions, bands, diverticula, binding down and strangulating the bowel.
(2.) Loops of the mesentery, omentum, &c., through which portions of the bowel pass; mesenteric, or meso-colic pouches, &c.
(3.) Tumours in the neighbourhood pressing on the bowel.
(4.) Enlargement of some abdominal organ, as the uterus acting in the same way.

This summary will sufficiently show how various and how numerous may be the causes of obstruction, and it also affords, on consideration, abundant evidence of the difficulties of the subject under review.

For practical purposes, the above list may, however, be greatly abridged. Considered clinically, these various sources of obstruction act either acutely—that is, causing rapid, sudden, and violent effects, and so calling for quick decision, as death ensues in three or four days if no relief is obtained; or they are chronic in their invasion and progress, it may be going on for weeks, and thus giving time for consideration as to what is best to do in the way of treatment.

This view for surgical purposes is both useful and practical, and classed in this way the causes may be thus arranged; but it is necessary to remember that chronic cases in their progress may become acute, and acute cases may cease to be violent; yet the general statement is true, that one set of cases occasions rapid collapse and death, while the other may be prolonged for days and weeks.

Causes of acute obstruction:—Volvulus; internal hernia; intussusception (usually, but not always); foreign bodies, occasionally; bands, adhesions, and loops.

Causes of chronic obstruction:—Foreign bodies, as a rule; prolonged constipation; stricture from ulceration; growths, as cancer, tumours pressing on the bowel; cicatrices from wounds; chronic tubercular inflammation; and rarely, yet occasionally, intussusception runs a chronic course, weeks passing before urgent symptoms arise.

It is unnecessary to dwell at any length on the obvious necessity of employing every available means of localising and defining the nature of the obstruction before any operation is thought of. It is true, we sometimes utterly fail in doing this; but more usually we are able to recognise one source and place of obstruction as being more probable than any other.

In no ailment is a clear, detailed, and well arranged clinical history more necessary than in dealing with this affection. To secure this much patience and perseverance are often required. It is well to commit this history to writing when dealing with obscure cases, as it may be necessary to ponder and consider the whole grounds on which an opinion is to be formed again and again before acting. The family as well as the personal history must be minutely investigated, as great aid is often got from knowledge of the family tendencies of the patient. What seems to him a very trivial circumstance in his own history, or in that of his relations, may throw the greatest light on his present condition. The patient's constitutional proclivities; the presence of the tubercular or syphilitic diathesis; the state of the health before the present ailment set in, especially as regards the abdominal organs and their functions; the action of the liver, kidneys, and bowels; attacks of gall stone; enteric fever or peritonitis occurring at any former period of his life; his liability to diarrhœa, or dysentery, or constipation, or colic; the passage at any time of blood or slime; the former existence of hernia; any history of abdominal wounds or injuries; the existence of any movable body in the belly—these are all points which require consideration. If the patient is a female, it will be necessary to obtain the history of her uterine condition; and, finally, to know in every case whether narcotics have been much used.

Further, we must get the most careful account of the invasion and progress, from day to day, of this and any similar attack. We must observe with care his pulse, his condition as to emaciation and cachexia; we must give full weight to the age, posture, expression, demeanour, and strength; if there has been any vomiting it will be necessary to define its relationship in time to the other symptoms, also its nature and persistence; as to pain, its degree, time of its first occurrence, its acuteness, continuance, and exact seat. These are all points of which we must have a clear conception before we proceed to the personal examination of the patient's body in order to localise, and, if possible, detect the source of the obstruction, and so to determine, if need arise, what operation should be performed. I may here add that in such personal examination, if we wish thoroughly to explore the rectum or palpate the abdomen deeply, chloroform is capable of affording the greatest service.

On the subject of age I would remark that a consideration of it will often be of great use to us. Intussusception may occur at any age; but it is most common in early and late life. Bands and inflammatory adhesions are most frequently met with in youth; twists, again, tumours in the neighbouring organs, gall stones, stricture, and also inflammatory adhesions occur in middle life; while malignant structure, thickening from old reducible hernias and internal strangulation by twists, and impaction in a pouch are the forms of obstruction mostly met with in the old.

Taking all cases and ages together, it is found that bands and adhesions are the most prevalent sources of intestinal obstruction, while intussusception comes next, and then malignant structure.

As to the parts of the abdomen in which these various causes of obstruction lie, and, consequently, what will be found to be the most direct and effective way of getting at them, I may say that if we leave invagination out of account the most common seat of acute obstruction is in the small bowel, and of chronic closure in the large gut. Invagination occurs in both large and small bowel, and so may lie in any part of the abdominal cavity; but it is near the extremity of the small intestine, and at its middle, that intussusception has been most usually found, and the invaginated portion may pass so far down as to be capable of detection in the rectum. It is in the right iliac region, then, that the free bowel will be most probably found. It is, in short, at the ileo-cæcal valve that in the majority of cases we may seek for the place of invagination. Success in operation for this dire affection is, however, lamentably small, whatever method we use, as the patients are for the most part so young and weak, and have usually been so greatly exhausted by suffering when the question of operation arises, that we are helpless to save them, even supposing inflammatory adhesions have not welded the coils of the bowel together so as to render them incapable of being disengaged.

Loops, bands, adhesions, and diverticula lie also mostly in the right groin, and are to be sought for there. It is the small bowel which is almost always involved in these conditions. The vermiform process is not uncommonly the imprisoning agent. Twists of the bowel (on itself and round the mesentery as axis) are commonly found to involve the ascending colon and the cæcum, and after that the sigmoid flexure is most commonly the seat of this dangerous condition. This affection, then, lies mostly in the right groin also, or if not there, in the left groin, and sometimes it is the small bowel which revolves round its mesentery, then the obstruction will lie in the centre of the belly. If the ascending colon and cæcum be the seat of twist, then there will be marked distension, and this may be one-sided, while fluids can be passed high up by injection and traced in their passage up to the right groin. Simple stricture arising from cicatrisation, or tubercular or syphilitic ulceration, or the effects of inflammatory action, may (but rarely) occur high up in the small bowel. It is in the colon that contractions after dysentery are chiefly seen, and it is, as Rokitansky shows, after tubercular affection, and not in consequence of the ulcerations of fever, that narrowing of the gut is most apt to come on. No special region of the abdomen can be looked on as that in which search is specially to be made for these contractions.

Malignant stricture occurs most commonly in the large gut, and especially in the rectum and sigmoid flexure. It is epithelioma for the most part which occasions such narrowing; but any of the different forms of carcinoma may be met with in the bowel.

Old hernias cause thickening, and so contraction may, of course, exist at any of the usual apertures. Internal strangulation by the bowel slipping into pouches may locate itself in any part of the cavity.

From this short review, then, of the localities or positions where the most ~~common~~ causes of obstruction are seated we ~~get this information, that~~ That of the six most important sources of obstruction—viz., intussusception, loops, bands, &c., twists, simple and malignant stricture, and internal strangulation by pouches, the three first are most frequently found in the right groin. Simple stricture is also, in a certain proportion of cases, situated there; while malignant stricture is far more commonly seated in or below the left groin; and internal strangulation has no special region where it can, in the majority of cases, be looked for.

The practical bearing of these observations will be apparent shortly. Now, let me very briefly review the different surgical operations which have been, and still to some extent are, practised for the relief of obstruction, and then we will be the better able to understand which of these procedures is best fitted to meet the different kinds of obstruction we have to deal with.

~~There is a~~ plentiful choice ~~of operations~~, First, the abdomen may be opened in the middle line, or outside of the recti on either side, or a descending coil of bowel may be cut down on wherever it shows itself. The first is, on the whole, the operation which fulfils the indications in many circumstances, as it gives freest access to the whole cavity, and there is less risk of effusion between the muscular walls of the belly, and perhaps affords the best hopes of a rapid recovery. "Laparotomy," as it is now termed, is, however, a very serious and difficult operation in cases of intestinal obstruction. As is well known, it is in no way comparable to opening the belly to remove an ovarian or other growth, as not only are the patients on whom it falls to be performed usually much exhausted by the nature and continuance of their complaint, but the bowel is so much distended that its return within the cavity of the abdomen after the parts have been examined is always most difficult (in truth sometimes almost impossible), while the manipulation which is required to attain this end immensely increases the risks of the operation. The tension too which exists after the belly has been closed is so great that all hope of that rest and freedom from irritation, which is essential to the successful treating of such wounds is often destroyed. It is not a little remarkable how, before the belly is opened, in many cases the distension of the bowel may not appear great, and there may be no great prominence of the abdomen; but, so soon as the wall is divided, coil after coil of greatly enlarged and gas-distended gut come out, defying all means of repression or even protection, and causing the utmost dismay as to how best to deal with them. It is always most desirable not to puncture the distended bowel, as however small the instrument employed, there is always considerable fear of fæcal exudation, and ligatures often fail to bring the serous surfaces together so as to close the aperture. The walls of the bowel, too, are thin and weak from the distension to which they have been exposed, and so very serious damage may result. It not unfrequently happens that distinct rupture of the gut wall takes place where the puncture has been made. In replacing the extruded bowel, take what care we may, it is apt to be bruised and injured, and small extravasations of blood will now and again appear and escape into the cellular connection when pressure is withdrawn. The contact of the finger nail and finger tips must be carefully avoided, and sponges introduced between the hand and the bowel; but with all this and every care we can use harm is too sure to follow. There can be little doubt but that it is the manipulation which does such serious harm in laparotomy, and it is probably this which renders the operation so disastrous. In ovariotomy and similar operations we do not, as a rule, require to touch the bowel, and occasionally it is never seen. Further, laparatomy is, in many cases, the only available operation in obstruction occurring in very young and weakly children, yet they are wholly unable to stand so terrible an ordeal.

The cases in which laparotomy is indicated are those in which we have to do with tumours which we desire to excise at the same time that we relieve the obstruction which they occasion; in cases of intussusception, if it is thought possible to disengage the invaginated bowel (that is—early in acute cases if it is to be done at all); in twists also, and in strangulation from internal hernia when we have to adjust the parts, and not merely to save life. It is probably the only operation by which a foreign body obstructing the bowel (not being fæces) can be extracted. In occasional instances in which the exact position of loops, bands, or adhesions can not be recognised, laparotomy may be our wisest proceeding, but of this more will be said. Cancerous tumours, if of small size, and situated in the small bowel, may thus be excised, and the bowel re-united, or a false anus established. If the tumour be in the large bowel, then after excision a false anus may be established in the loin or in the groin, as, it will be in the recollection of the president, I attempted in a patient I saw with him. This is better than attempting to unite that part of the bowel, but in the small gut suturing the divided bowel has most to be said in its favour. It is, of course, only in limited strictures that this operation (colectomy) can be attempted. By such an operation, we attempt not only to remove the obstruction but get quit of the disease. So laparotomy has this strong claim on our attention that it may enable us to carry out a curative, and not simply a palliative treatment.

When laparotomy has to be performed, it is best to make at first a small opening to enable us to search for the seat of obstruction without opening the whole cavity. This must be done with the greatest gentleness; afterwards, if it is found necessary, the orifice may be enlarged. It is the empty bowel which we seek for, and this lies in the pelvis. We trace it up to the place of obstruction. This plan is better and easier to accomplish than to pursue the opposite course, which is what is commonly done. As little bowel as possible should be exposed. A sponge wrung out of carbolic solution should be kept over the hand and wound. We should search first in the neighbourhood of the cæcum, and determine whether the mischief is in the greater or the lesser bowel. The cæcum is the best starting point in all these investigations, and is a good landmark for our proceedings.

If there is a portion of the bowel in a loop, or under a band, or in a pouch, we should draw on the lower or empty part, and this way experience has also shown to be the best way of disengaging an intussusception. If we have to deal with a small band we may break or cut it, but it is better to ligature a broad or large band at two places and divide it between the ligatures, as it is difficult and troublesome to secure any vessel which may bleed.

If a foreign body is to be removed the portion of the bowel containing it should be drawn well out of the belly. After the substance has been extracted a false anus may be established by stitching the open gut to the superficial wound, or in favourable circumstances, the bowel may be united by suture.

So much for laparotomy. It is, I repeat, an operation not to be undertaken with a light heart, or in any but the most desperate circumstances. There are other methods of operative relief which are much preferable, if they can be used. Of these, colotomy in the right or left loin would, of course, be chosen if we are so able to localise the obstruction as to be sure of opening the bowel above it.

In incurable obstruction seated in the rectum and sigmoid flexure—that is, low down, left lumbar colotomy (or Callisen's operation as it should be called), is the operation which beyond doubt would be employed; but it is sometimes far from easy to be sure that by that operation we can get quite beyond the obstruction even in cases in which we seem to have clear evidence to support that conclusion. The descending colon has been often opened after very careful examination, and the obstruction found to extend above the point opened. That the bowel can be safely and comparatively easily reached in either loin is well established, and attempts have been recently made to revive Littre's operation (in which the sigmoid flexure is opened in front through the peritonæum, that is, in the left groin); yet all the difficulties connected with getting beyond the disease are in that operation much enhanced by the near neighbourhood of the disease even when it is confined to the rectum. The necessity of opening the peritonæum in operating will make surgeons slow to substitute an operation which was in former years condemned for its unfavourable results, for one which possesses so many stronger claims to success.

If the obstruction lies in the descending colon, left lumbar colotomy (Amussat's operation) is that which should be preferred. No one has yet proposed to re-introduce Fine's operation, in which the transverse colon (which is surrounded by peritonæum) is opened; but in these days of obtrusive and restless innovation there is no saying what may yet arise. It is certain that in an immense proportion of cases of intestinal obstruction taken overhead, that the obstruction lies below the right loin. Bryant gives the proportion as 15 to 1.

In the many cases in which the obstruction lies about the caput cæcum and the ilio-cæcal valve, Nélaton's operation (which is an extension and improvement of Pillore's) is beyond doubt the best surgical proceeding. Pillore opened the cæcum, but Nélaton demonstrated how by a very small incision in the right groin that region could be perfectly examined, and how with little risk life could be saved in irremediable cases by opening whatever coil of the bowel (necessarily the distended part, and so above the obstruction) protruded at the wound. The success of this operation has been great, and in the cases where I have myself had recourse to it, it has been most satisfactory in saving life. If the obstruction is such that it cannot be removed, or if the steps necessary for its removal (from the condition of the patient) involves too much risk, this is a most invaluable operation. No blood-vessel need be wounded. The bowel is not much handled or exposed, and an outlet is certainly secured for the imprisoned fæces. It is quite true that in most cases this is only palliative—that is, it does not remove the cause of obstruction in all cases, *but it saves life.* I hold it places the artificial anus at the most satisfactory place —a much better place than in the loin, as it is under the control of the patient it can be dressed by himself without aid, and an apparatus can be best applied to restrain discharges.

Finally, I hold that in the considerable residuum of cases in which we have done our best to determine the seat of obstruction, and have failed, that this right inguinal enterotomy is the right operation to perform.

In conclusion, I would say that, as a rule, if an operation for intestinal obstruction is to be performed,~~—if the physician has retired discomfited and the surgeon is asked to interfere~~, the sooner ~~he does so~~ the better. In acute cases it is a question of hours, and in chronic cases delay beyond a week is inexcusable. Sometimes, as in a recent case which I saw with Dr. Hugh Miller, our hand is held by the knowledge that similar attacks in the same patient have, after as long intervals, without interference passed off. But, as a rule, if internal remedies intelligently and perseveringly administered have failed to bring relief, then no good but only evil can come of delay. The using of purgatives should be by the rectum alone; and nourishment, too, as is well known, can be largely administered by the bowel, and it is well that full advantage should be taken of this knowledge. Exhaustion, peritonitis, and perforation are imminent, and the mere hopelessness of the patient will so oppose success that the operation can only lead to disaster. The length of time the obstruction has existed is not so good a criterion in determining whether we should operate, as the violence of the symptoms and, above all, the persistence of the vomiting, which does so much to exhaust the patient. Vomiting, pain, distension, are perhaps the most threatening conditions, and if they continue, are the strongest arguments for operation. If the obstruction is from some mechanical cause, medicine can be of little aid, and the knife alone be of real service. If the surface is already cold and bedewed with sweat, the face pinched, the pulse weak and intermittent, the belly tender, and the courage gone, we should not interfere. The case is hopeless, and we are too late. Twists, internal hernia, and intussusception in very young children are almost always fatal, do what we like. Bands and chronic strictures are more encouraging, so far as operation goes.

In the preceding remarks I have carefully avoided reference to statistics, though these are abundant; but I have tried to embody the results of their teaching. I have not spoken of the use of the long tube, or of enemata, important and invaluable as these means of treatment in some cases are, because I desire to confine your attention as much as possible to the one point, the "choice of operation in intestinal obstruction." But I may say of the use of enemata in these cases the patient should be laid on the right side with the knees drawn up, and the fluid injected through the tube as it is passed, so as to facilitate its entrance. I would also desire to express in the most emphatic manner my conviction that, in cases of intestinal obstruction, purgatives kill many persons—if not by intensifying and aggravating their condition before the operation is performed, certainly afterwards by the violent action of the bowel to which they give rise after it is set free. The well known rule of administering no solid food and only opium, and as little liqnid as possible by the mouth should be strictly adhered to.

True it is that not a few cases of intestinal obstruction which seem altogether hopeless recover by the unaided powers of nature, and in some of these instances we cannot tell what was the cause which gave rise to the stoppage even after it has passed away. But, alas! these rare cases of recovery are too often allowed to warp our judgment and cause us to stand aside and abstain from interfering when an operation alone can be of any use. The great mortality which has attended operations for obstruction has had a pernicious influence upon surgical practice. We hesitate in the face of such terrible statistics of failure as past practice has recorded. But it may be that it is chiefly in this delay that the danger lies, and that bolder action might be the means of averting complications in these most unfortunate and anxious cases.

I am very conscious that from want of time and the pressure of many distracting duties during the last few days, it has been impossible for me to put these observations before you so fully and satisfactorily as I would have wished. I have omitted much I would have liked to add; but I hope you will find in these remarks materials for discussion, and I will heartily rejoice if it tends to throw light upon the line of action to be pursued in what are always trying and often helpless conditions.

Discussion on Dr. Macleod's Paper.

Dr. George Buchanan said that intestinal obstruction could be looked at from two points of view; first, as referable to mechanical causes, as had been done in the paper just read, which was one characterised by marked ability and thoughtfulness; and secondly, to causes which could not be so named. The great practical difficulty in connection with this subject was the determining in each case whether the symptoms arose from some mechanical obstruction, or from something of the nature of functional derangement of the bowel itself. Every practitioner was familiar with cases of obstruction, accompanied even by stercoraceous vomiting, and presenting every sign of obstruction arising from some mechanical cause, which yet had become spontaneously relieved. As a fact of familiar experience not a few cases terminated in this way. But, to decide in every given case whether the obstruction did arise from mechanical causes or did not so arise, was often a matter of the very greatest difficulty.

When the surgeon was certain, or nearly certain, that the obstruction did not arise in the great intestine, then the choice must be between opening the abdomen and Nélaton's operation for artificial anus. The difficulty of the choice was almost insuperable. If the obstruction were mechanical, then in opening the abdomen the surgeon not only relieved the urgent symptoms, but cured the case; while, on the other hand, still on the assumption that the cause was mechanical, if he performed Nélaton's operation, he left the patient relieved, indeed, but with the excessive permanent discomfort consequent upon the operation. The relation of one or two cases in his own experience would bring out the view more clearly.

He was called to see a case of Dr. M'Carron's, of acute obstruction, with stercoraceous vomiting and collapse. He performed abdominal section. He carefully searched the whole abdomen and could find no point of obstruction; but in several places the bowels were glued together by recent inflammatory effusion. This was undone, the abdominal cavity washed out, and the patient made a good recovery. The second was a case of Dr. Smith's (and was also seen by Drs. Leishman and Gairdner). The obstruction was accompanied by stercoraceous vomiting. On opening into the abdomen no local cause whatever could be detected. Neither disease nor abnormality of any kind appeared to exist; and on *post mortem* examination the bowels were found perfectly normal. These cases showed how very difficult the diagnosis must be. If there were anything in the symptoms which would point to a band or internal strangulation as the obstacle, he would certainly prefer abdominal section; in the absence of all such signs he would open the small intestine by Nélaton's operation.

Dr. Knox made a few remarks, chiefly in regard to the difficulty of so diagnosing the case as to have any adequate idea of the risks of operation.

Dr. Hugh Thomson said that, when a young practitioner, he had been called to see a case of obstruction, of the nature of which he had this indication, that he had relieved the patient of stricture of the rectum on a previous occasion by passing a catheter. When he saw her on the last occasion rupture of the bowel was imminent. He recommended puncturing the colon, but the consultant declined to take this course. On the woman's death he made a *post mortem* examination, and satisfied himself that it would have been an easy matter to open the bowel in the left loin without involving the peritonæum. Indeed, the bowel forced itself through the section. It was a case of cancerous disease of a limited kind; and if Amussat's operation had been made it would have saved her life. In another case he had found the stricture in the sigmoid flexure so complete that not even a bristle could be passed through it.

The President said that these cases were certainly most puzzling. In those that he had seen he had been struck with the extreme difficulty of forming an opinion antecedently to operative interference as to what they might expect to find. The last three cases which he had to do with well illustrated this. The first was a case in the Western Infirmary, where there was absolute proof of the existence of obstruction for eleven days. The surgeon who was called rather urged an operation, and that even hours were of the greatest importance. The operation was performed at once. The tract of the small intestine was taken out in successive loops. It turned out to be a case of cancerous disease of the ileo-cæcal valve, in which the operation was of no use, and only accelerated the otherwise certainly fatal issue. The next case was at a distance from Glasgow, where he had not the full command of surgical aid. Notwithstanding the greatest difficulty in the diagnosis it appeared to be a case for operation. No operation was performed; but on *post mortem* examination it was shown that it was exactly a case which an operation would have cured. The third case was that referred to by Dr. Macleod as occurring in Saltcoats. It appeared to be of the gravest nature, and at one period they thought a fatal termination was imminent, death being averted by the use of belladonna. As the advice of surgeons in Glasgow was greatly desired, Dr. Robertson took the step of bringing the patient to town, and succeeded in bringing him in safety on his own responsibility. Two days afterwards a spontaneous cure occurred. In 1861 he published in the *Lancet* a case of obstruction in a young girl in the wards of Edinburgh Royal Infirmary. The symptoms extended over a fortnight. Mr. Spence, who was called in to see her, was very unwilling to operate. There was enormous distension of the bowel, so that the wriggling of the bowel was seen through the abdominal wall. At the end of the fortnight the case terminated in spontaneous recovery. In the record of the case, as published, the state of matters is graphically put in the statement that the first matter passed through the bowel was so exactly similar to the last matters vomited that he absolutely could not tell the one from the other.

Dr. Macleod, in his remarks in reply, adverted to the very small proportion of recoveries in cases operated upon by him by abdominal section. With Nélaton's operation he had been more successful. But, taken as a whole, surgical operations for abdominal obstruction had been disappointing. In regard to the abdominal operation, several years ago, had he been asked his opinion, he would have given it in favour of it; but experience had made him change his opinion. The operation was an extremely dangerous one—the chief element of difficulty being the returning of the bowels afterwards. Dr. Macleod gave some cases in his own experience, illustrative of the difficulty of replacing the bowel. Nélaton's operation, on the other hand, was not a dangerous operation, and there could be no doubt that death would be averted in not a few cases by its adoption in preference to abdominal section.

Session 1883-4.

Meeting IV.—*11th January, 1883.*

Dr. Gairdner, *President, in the Chair.*

DISCUSSION ON CATHETER FEVER.

The President said that, in view of the interest in the subject of "catheter fever" which had been evoked by the address lately delivered to the Medical Society of London by Sir Andrew Clark, he had suggested this as the subject for that evening's discussion; and at his request Dr. Cameron had undertaken to open it.

Dr. H. C. Cameron

spoke as follows:—In responding to the request of our President, it is my intention, while drawing to some extent upon the recorded experiences of others, to endeavour to regard the subject to a large extent in the light of my own experience. It is now a good many years since Sir James Paget, in a lecture on "The various risks of operations," reminded the profession that the passing of a catheter was by no means always an unimportant procedure. "For a first catheterism," he wrote, "has been the first step towards death for many old men. . . Look upon first catheterism as involving a risk of troubles as great as that of amputation of a finger or a toe, or the removal of a small tumour, in a person of the same age." There is reason to believe that this truth is not even yet so thoroughly appreciated as it deserves to be. The risk is often very serious, apart altogether from the mode in which the operation is performed. Sir Andrew Clark has lately, in an address delivered at the Medical Society of London, again called attention in more forcible terms to the same fact; and it was, I believe, in consequence of the interest which this address has aroused in all who have read it that it has seemed well to our President to elicit by discussion here to-night the views and experiences of us in Glasgow in reference to this important matter.

My first duty is to clear the way for the discussion, by considering what is meant exactly by "catheter fever," as it appears to me that a good many unfavourable conditions which follow upon urethral instrumentation, but which have little else in common, are apt to be confounded, and dealt with under one designation.

I. Let us consider some disastrous effects which follow upon the relief of a distended bladder by means of the catheter. Let us suppose the case of an elderly man, who has been apparently in the enjoyment of good health, but who begins to complain of frequency of micturition. He has no actual fit of retention, but rather frequent calls to urinate, more especially during the night. Some day, his troubles having culminated, he consults a surgeon, who finds that he has to do with a man suffering from enlarged prostate, and who for weeks, or months it may be, has been unable to empty his bladder, and has only succeeded in warding off the urgency of complete retention by the frequent passing, night and day, of small quantities of urine. If the catheter be introduced, a large quantity of urine will, of course, be withdrawn; and the immediate effect will be a great relief of the patient's symptoms. But the first disastrous effect which follows such a procedure, in a certain number of cases, is the occurrence of hæmorrhage into the bladder, and this no matter how carefully and successfully the instrument may have been employed. This sequel of catheterism appears to be confined to old age and prolonged retention, and is, I have reason to believe, often the starting point of deterioration in the patient's general health, leading it may be to a fatal issue. So frequent is hæmorrhage after catheterism in all kinds of cases from rough or faulty manipulation, that the correct explanation of the clinical fact I have just referred to is apt to be passed over, and to escape the attention of the operator. I may refer to two cases in which this occurrence was well marked. The first of these was an old gentleman whose urine I was asked to withdraw one morning by Dr. Finlayson (whose attention had previously been directed to this subject, and who lately exhibited at the Glasgow Pathological Society the bladder and kidneys of a patient who had thus suffered), and from whose bladder I obtained a large amount of clear limpid urine. I used a soft catheter, which passed in without a hitch; and neither during the flow of urine, nor after the withdrawal of the catheter, was there the slightest appearance of blood. In the evening I visited him again, and finding that he had been unable to pass water in the interval, reintroduced the catheter. On this occasion, from first to last, the urine was extremely bloody; the blood having evidently flowed into the bladder in the interval, either from its own walls or the kidneys, or both. I may add that the urine continued bloody for several days, but ultimately became clear, and was spontaneously voided. The attack of retention seemed an almost accidental one in the course of a prolonged illness, which ultimately proved fatal; and there is reason to believe that the bladder had been more or less distended for some time before I passed the catheter.*

A few weeks ago I met with a very similar case in the practice of Dr. W. L. Reid. An elderly gentleman had been confined to the house for a fortnight or so with symptoms principally referred to the abdomen, such as uneasiness in that region, obstinate constipation, and frequency of micturition. He was very obese, but we satisfied ourselves on examination that he had a big prostate and a distended bladder. I withdrew, by means of a soft catheter, and without difficulty, the greatest amount of urine I ever took off at

* I had, on a previous occasion, sounded this old gentleman's bladder without the slightest bleeding or other inconvenience.

one time from the male bladder. No trace of blood was visible either in the urine or in the catheter. He felt great relief for a good many hours, but during the night became restless and pained, and the next urine from the bladder was largely mixed with blood. His troubles thickened quickly; he required the aid of the catheter nearly always; the urine, in spite of every precaution, became altered in character, ammoniacal and mixed with muco-purulent material. He became thirsty and feverish, and continuing to grow worse, died in a week or two afterwards. No *post mortem* examination was made.

There seems no doubt (and the fact has been long well known to a few, if not heeded by the majority of surgeons) that hæmorrhage in such cases follows from the rupture of diseased and over-distended blood-vessels in the pelvis of the kidney and the walls of the bladder, by the sudden and rapid withdrawal of the counter-pressure of the retained mass of urine.* Analogous forms of hæmorrhage are not wanting in other fields of surgery. I might cite many examples, but will content myself by reminding you of two. One is referred to by Percival Pott in his *Treatise on the Hydrocele.* An old hydrocele may be tapped in an old man without the slightest appearance of blood in the fluid from first to last. It re-fills rather rapidly, and on a second tapping the liquid is found in colour to resemble port wine or porter. This leads to the belief with many people that a vessel has been wounded at the first operation; but this explanation I have long been convinced is incorrect. Pott says:—"The vessels (especially those of the inner surface of the tunica vaginalis) are sometimes so large as to be very visible, and even varicous. If one of these lies within the way of the instrument, wherewith the palliative cure is performed, it is sometimes wounded; in which case, as I have already observed, the first part of the serum which is discharged is pretty deeply tinged with blood. Upon the collapsion of the membranes, and of the empty bag, this kind of hæmorrhage generally ceases, and nothing more comes of it." And he was also familiar with bleeding as a sequel of the operation without any wound of a vessel. "Upon the sudden discharge of the fluid," he says, "from the bag of an over-stretched hydrocele, and thereby removing all counter-pressure

* Since these remarks were made, Dr. Finlayson has directed my attention to an interesting article on the subject by Mons. Guyon in his *Atlas des Maladies Urinaires*, which is well worthy the attention of any interested in the matter. He figures a bladder with numerous ecchymoses and extravasations from this cause.

against the sides of the vessels, some of which are become varicous, one of them will sometimes, without having been wounded, burst. If the quantity of blood shed from the vessel so burst be small, it is soon absorbed again, and, creating no trouble, the thing is not known. But if the quantity be considerable, it, like the preceding, occasions a new tumour, and calls for a repetition of the operation." The other example of an analogous occurrence to which I may refer is that of intra-ocular hæmorrhage following the operation of extraction of cataract, during which a large escape of fluid vitreous has accidentally occurred. This is a circumstance well known to and dreaded by ophthalmic surgeons.

From what I have said, you will, I think, be convinced that one grave disorder, at least, that of smart vesical hæmorrhage in an old man, may occur on his first " entrance on catheter life." But another frequently follows (whether or not there may have been hæmorrhage), as in the second case I have narrated above. I refer to chemical fermentative alterations in the condition of the urine, within the bladder, rendering it often, by the end of the first week, turbid, ammoniacal, possibly bloody, offensive, largely mixed with pus and mucus; this being due to the introduction of septic agencies from without. When this has occurred, the patient too frequently sinks and dies with symptoms resembling those of septic poisoning. Indeed, I cannot help thinking (and I trust that in saying so I do not lay myself open to the charge of presumption) that the second of the two cases narrated by Sir Andrew Clark (*Lancet*, 22nd Dec., 1883) was rather of this character, than possessed of anything in common with cases of urethral or urinary fever from nervous shock, such as I shall refer to immediately. In that case, before death, there was great prostration, the face was yellowish and mottled, the lips dry and the breath fœtid, the heart's action was quick, the bases of both lungs were congested, the skin was sub-icteric, the patient was dull and heavy, the temperature of the body in the afternoon was 103°. At *post mortem* nothing definite was found outside the bladder, but the mucous lining of that organ was congested, and in part eroded, and everywhere coated with a greyish-white stinking mucus. Decomposition of the urine within the bladder during catheterism occurs for the most part in cases of old men and persons who, from fractured spine or other cause, have paralysis of the muscles of micturition, and even in these, certain antiseptic precautions, to which I need not further refer at present, will go far to avoid the difficulty. But I have also seen such putrefactive changes, accompanied by rigor, and other serious constitutional disturbance, occur in a case where the urethra was not interfered with at all, but where for three weeks a canula was fixed in the bladder above the pubes, showing that, at least in that case, the constitutional effects were not the result of instrumental irritation of the urinary passages. They were, I believe, caused really by the absorption of septic material from the bladder; into which the causes of putrefaction had been introduced.

II. But that grave consequences and even distressing disasters may follow the irritation and shock caused by passing an instrument through the urethra of certain persons is a fact well illustrated by other kinds of cases. First, a man may die within a few minutes of the operation having been performed, as he may after an incision in the soft parts, the extraction of a tooth, or the reduction of a dislocation, without anæsthetics. Some years ago, I had occasion to make an incision in the leg of an old soldier; and as he was very bronchitic, I persuaded him to dispense with chloroform. The moment the incision was made, he screamed, gave vent to a volley of oaths, and almost immediately expired. A *post mortem* examination revealed nothing beyond evidences of old bronchitis, and of advanced granular disease of the kidneys. A still more distressing case of the same kind, but due rather to mental emotion, occurred to me within the last few weeks. I was asked by a medical man to see an elderly lady with a small tumour in the groin which was causing her no uneasiness, but great anxiety. She was said to be the subject of fatty heart, prone to attacks of syncope, and with a weak and often irregular pulse. The tumour was evidently a small femoral hernia; and on trying to move it a little from side to side, it slipped with a gurgle from between my finger and thumb and passed up. I said to her; "Did I cause you any pain?" She replied, "Not in the least." I then asked her to feel the tumour now. On discovering that it had gone, she was evidently greatly startled and surprised, no doubt pleasantly so, for it had been much in her thoughts. Almost immediately she complained of feeling faint, and asked for some cold water. This was supplied her, but her syncope deepened, and in not more than about ten minutes, I think, life was extinct. These are examples of shock following respectively upon sudden pain and sudden mental emotion. Any of us may meet with the same occurrence any day in passing a catheter in the case of a patient with a weak and flabby heart. In a well known essay, often referred to in the literature of this subject—"On certain rapidly fatal cases of urethral fever following catheterism," in the *Edinburgh Medical Journal* of 1871, Mr. Mitchell Banks narrates a case of this sort as follows:—

"A good many years ago, a man between 40 and 50 years of age, and by his looks, much broken in health, came to the Liverpool Royal Infirmary. He complained of some urethral obstruction, and one of the dressers proceeded to pass a catheter, but failed. Mr. Padley (now of Swansea), who was then resident surgeon, and who kindly gave me the particulars of the case, came to his assistance. He found that there was a false passage in the urethra, and with some little difficulty, but certainly without any undue degree of violence, he succeeded in passing an instrument (medium sized catheter, No. 6 or 7) into the bladder. There was very little pain complained of, there was no bleeding, and indeed nothing to indicate that there was any lesion of the urethra, except the pre-existing false passage; but almost immediately after the entrance of the catheter into the bladder, the man was seized with a severe rigor. The instrument was at once cautiously withdrawn, but he passed into a state of profound syncope, and in a few minutes expired. At the time this occurred there had been considerable unpleasantness in the agitation with regard to the making of *post mortem* examinations at the Infirmary, and, most unfortunately, an autopsy could not be obtained; but, from the debilitated appearance of the patient, Mr. Padley had little doubt of the existence of a weak flabby heart."

III. In addition, however, to cases of ill-health and death following catheterism, whether they be caused by alterations of circulation, from the sudden withdrawal of long contained urine, or by septic mischief being introduced into the bladder; and in addition, also, to sudden deaths from the pain of urethral instrumentation acting probably on a weak heart, we have a class of cases different from all of these, and of great importance and interest to the surgeon, to which I should feel disposed to limit the name "urinary" or "urethral fever." These are cases in which irritation of the urethra, sometimes by instruments, sometimes in other ways, leads by reflex influence through the sympathetic nervous system, to attacks of a very sudden and sometimes alarming fever. The attack varies greatly in degree, being at times transient, at others more prolonged, whilst sometimes a fatal issue has followed in the course of a few hours. Mr. Banks, in the article already referred to, has narrated a case in which death occurred six hours and a half after the introduction of a bougie. He reports it as follows:—"In May, 1870, a man of about 30 years of age, a sailor, and apparently tolerably healthy, was admitted into the Liverpool Royal Infirmary. . . . . On the day after his admission, Mr. Puzey, the resident surgeon, examined him and found a stricture about an inch-and-a-half from the meatus, through which he could only pass a fine probe, which was left in for half-an-hour. Three or four days afterwards an attempt was made to pass Nos. 1 and 2 metallic bougies, but failed. However, a fine pointed bougie was passed, and left in for 10 minutes, after which 2 minims of Fleming's tincture of aconite were given. To show that the greatest care was used in treating the case, it may be mentioned that after each instrumentation the same dose was given, and an interval of 4 days was always allowed to elapse between each operation. On the next occasions Nos. ½ and 1 were passed through the stricture, and after these a fine catgut bougie, which was left in for an hour. When removed, it was found still tightly grasped, and on this and each succeeding occasion a stream of urine followed the withdrawal of the instrument. In this way the urethra was dilated, chiefly by catgut bougies, till the fatal day, when No. 4 metallic bougie was passed through the stricture into the bladder for the first time. It went quite easily, was removed in about 10 minutes, and was followed as usual by a stream of urine, but no blood came. This was about 12·30 noon, and the patient had the customary dose of aconite. In about half-an-hour he vomited his dinner, and soon afterwards had a rigor. At 3 o'clock he had another rigor, still continued to retch, and had a rapid pulse. Another dose of 2 minims of Fleming's tincture was given, and brandy and water, but this was soon vomited, and nothing could be kept down afterwards. He then began to complain of pain in the region of the bladder and loins, the pulse became quicker and weaker, and in spite of all that could be done the retching continued. At 6 o'clock he continued much in the same state, but an hour afterwards the nurse summoned Mr. Puzey, saying that the patient had had a sort of convulsive tremor, after which his appearance suddenly altered and he no longer took any notice after. By the time Mr. Puzey reached the ward he was dead, having lived 6½ hours after the introduction of the bougie. Eighteen hours after death I examined the body. The stricture was a long one, and commenced about 2 inches from the orifice. No injury whatever had been done to the urethra. The organs were all perfectly healthy. No congestion of the lungs. In the bladder was about a tablespoonful of thick muddy urine.

The kidneys were examined with great care. They were slightly congested, and on pressure a little thick turbid urine escaped from the calices, but beyond this no disease was found."

This ailment, whether slight or severe, is characterised by certain definite symptoms. It occurs only in the male sex. You may interfere to any extent with the female urethra; you may forcibly dilate it sufficiently to admit a finger or pull through it a large calculus, and no special constitutional disturbance need be looked for. It is also a disease of adult life. Children, though subjected occasionally to catheterism, and to prolonged use of the sound in cases of suspected stone, are not found to suffer from the affection we are considering. It is indeed entirely confined to adult male life, when the virile powers are active and the liability of the organs in question to nervous shock may be considered greatest. After some irritation applied to the urethra (very often in the first passing of urine thereafter), the patient is seized with vomiting, with rigor, sometimes frequently repeated, with severe frontal headache. The conjunctivæ become injected, the temperature is high, and the urine scanty, if not entirely suppressed. This represents, of course, a smart attack of urethral fever; but its more common manifestation is merely a slight chilliness and feverishness lasting for an hour or two. It may, however, as we have already seen, be so severe as to prove fatal in a few hours, or in other cases after a lapse of some days. As regards this form of ailment, I cannot think that the term "catheter fever" is a happy one. It has no special connection with retention of urine and the use of a catheter. Indeed, I have never known it follow the passing of a catheter for the relief of a painful fit of retention of urine.* It occurs frequently after the passage of bougies through a strictured urethra for the first

* I have had a large experience in passing the catheter in consequence of isolated fits of retention, both in cases of organic stricture and in others; and, as I say, I can recall no instance of such interference being followed by urethral fever. This may simply be an accidental experience on my part; but it may, I think, be due to the fact that at the time of the operation the patient is suffering and groaning with the agony of a distended bladder, and the effect produced on the nerves of the urethra by the instrument may (even in cases otherwise susceptible) be much modified and discounted by the counter-irritation present in the nervous disturbance caused by the distended bladder. At all events, the circumstances are clearly different from a case in which the patient is easy and comfortable at the moment, so far as his urinary organs are concerned, and in which one only passes an instrument for the purpose of dilating a strictured urethra or searching for a stone. In such a case the only decided impression on the nervous system is that produced by the instrument. There is no other co-existent as in the former instance.

time; or, after the passage of a larger instrument than usual in a urethra habituated to the use of instruments of smaller size. I have a patient through whose urethra I pass Nos. 9, 10, and 11 bougies every second Sunday, and who has had instruments regularly passed since 1835—nearly half a century ago. During all these years, although he has occasionally felt chilly and out of sorts after instrumentation, he has only experienced severe urethral fever on two occasions; and on both of these a sudden advance was made in the size of the instruments passed. The ailment is often met with after the operation of sounding for stone, no matter in how short a time this may be accomplished. But it also occurs apart altogether from instrumentation. I have known it follow the passage through the urethra of a calculus; and in the case of one patient, it often succeeded the passing of a quantity of uric acid gravel. It is met with in its severest forms sometimes in patients who have been subjected to the operation of lithotomy, at the stage when the urine at first flows again by the urethra. Last May I cut a young man, aged 18, for stone. He progressed satisfactorily until the day on which he first passed urine per urethram. In a few hours afterwards he was seized with a most severe rigor, his temperature reached 105·2°, and his urine became for many hours almost entirely suppressed. In 48 hours, however, he was again quite well, with a normal temperature, and recovered without any bad symptoms. I hand round for your inspection a chart of his temperature during his illness.

In the discussion which took place at the International Medical Congress in London in 1881, on "The relations of minute organisms to unhealthy processes arising in wounds and to inflammation in general" (*Trans. of Internat. Med. Congress*, vol. I, p. 312), Professor Lister incidentally related a case of this sort, which proved fatal. He said—"Let me say a few words in the first place with regard to acute inflammation. Acute inflammation is certainly very often caused by the products of decomposition, the results of fermentation induced by the development of minute organisms. Of that we have abundant proof; but we have also sufficient evidence, as it appears to me, that inflammation is often caused otherwise—viz., through the nervous system; that, in fact, the ancient principles of John Hunter, with regard to sympathy, were true principles. Let us take, for example, the case of irritation of the urethra, leading to disturbance of the kidney, without any affection of the intermediate parts; the irritation applied to the urethra, the effect produced upon the kidney. This, I conceive, can only be explained through the nervous system. For instance, I have seen a strong and apparently healthy patient, on whom lithotomy had been performed, go on perfectly well until the first occasion of the passing of urine through the urethra, as the wound healed. He then had a violent rigor, and from that moment there was complete suppression of urine. Death occurred from uræmic poisoning, and upon *post mortem* examination I found what had apparently been previously perfectly healthy kidneys affected throughout with most intense inflammatory congestion. That I can only explain as occurring through the nervous system, by virtue of nervous sympathy existing specially between one part of the urinary tract and another."

In the days when stricture was treated by the application of caustics this sequel of the operations seems also to have been well known. Sir Everard Home, in his "Practical Observations on the Treatment of Strictures in the Urethra," refers to this, and narrates some cases in illustration of it. At p. 483 he writes under the heading "Ague":—

"That the application of the caustic to a stricture should bring on a regular paroxysm of fever is an effect that could not be expected, and one for which it may appear difficult to account. It is most frequent in patients who have been long in hot climates, and has in general been occasionally a symptom of the disease in its early stages, when the patient was under the influence of any temporary indisposition. I have, however, met with this symptom in patients who were never out of England, and had no recollection of having before experienced it. The paroxysm of fever seems to arise from the stomach sympathising with the stricture, and purgative medicines are the most effectual means of carrying it off. . . I have in no instance seen any serious consequence arise from these attacks, though in several cases they have been very violent. In general, they come on when a stricture is destroyed, being found to occur less frequently at any other time."

He then details, in illustration of these statements, several well marked and typical examples of what we would now-a-days call urinary or urethral fever.

I have left myself little or no time to speak of treatment.

In cases of catheterism for prolonged retention in old men, it will be admitted, from what I have said, that there is a necessity for caution as regards the withdrawal of the urine. This should be done slowly; and if in large quantity should not all be withdrawn at once. Further, one cannot be too careful in regard to the condition of the instrument used. Besides being scrupulously clean, it ought to be washed in carbolised water inside and outside, and smeared with carbolised oil (1 to 20) before its introduction; while care should be taken that no sudden rush of air takes place through it into the bladder, by closing the end of the catheter with the thumb of one hand before relaxing the pressure which the other may be making on the lower part of the abdominal wall.

For genuine attacks of urinary fever following upon irritation of the urethra by any of the causes to which I have referred, I know of no satisfactory treatment by drugs. I have never had good cause to entertain a belief in the benefits of quinine, aconite, or bromide of potassium, whether used as prophylactic or remedial agents. The administration of anæsthetics, during instrumentation, if it sometimes prevents, also often fails in preventing, the onset of rigors and fever. But we must all be convinced of one fact—viz., that it is undesirable that any patient should be exposed to cold or fatigue after an instrument has been passed, especially if the urethra be unaccustomed to its use. It is unwise to sound bladders in one's own consulting room, especially if a long walk, cab drive, or railway journey has to follow. The slightest chilliness after instrumentation, especially if accompanied by a diminution in the amount of urine, or by vomiting and frontal headache, should at once be dealt with by confinement to bed, and by large warm applications (fomentations or poultices) to the loins. Fortunately, a fatal issue is rare; and under such simple means as I indicate recovery will usually be satisfactorily accomplished.

Dr. Macleod said the subject had been admirably brought before them by Dr. Cameron. He thought, however, he had done unwisely in excluding the question of treatment, which was certainly as important as the matter of diagnosis. As a hospital surgeon, he had given special attention to the treatment, and these cases had now lost much of their terrors for him. He was not aware of any fatal case of urethral fever having occurred in Glasgow, though any one seeing the affection in its more violent forms must be struck with its severity.

The term, "catheter fever," was certainly as bad as could be, as it was not possible to separate fever after catheterisation from the larger question of this affection originating in other ways in the genito-urinary organs. No doubt the phenomena, as a matter of fact, did come as a consequence on the surgical treatment of the urethra. He quite coincided in Dr. Cameron's opinion that the various morbid conditions often included under the one general term had no connection beyond that of coincidence. The cases of very sudden death—occurring as if the patient had been struck on the head—must clearly be set down to shock. In another class of cases the affection was clearly of a septicæmic character. Dr. Macleod traced the symptoms in detail, to prove that they were precisely the same as those of blood poisoning. How were these cases to be accounted for? From the toxic influence of decomposed urine in the circulation; the urea becoming decomposed from retention in the bladder. He preferred the term urinary fever to any yet suggested, and he thought that it might be divided into three kinds—(1.) The ordinary form, beginning with rigor, and sometimes with sweating, and generally passing off in a few hours; (2.) the pernicious form, in which there was a prolonged cold fit, and the patient might die before reaction set in; and (3.) the remittent form. In regard to the question of treatment, the first thing to be kept in view was the absolute necessity of never dealing with a case of this kind except with the greatest care and attention. In passing the catheter they must bear in mind that there was always a certain amount of risk. The urine should be carefully examined, and the preparatory matters carefully attended to. He had great faith in chloroform. In the case of a surgeon, a Dutchman, who was lately under his care, he was told by the patient that the passing of a catheter would certainly bring on an attack of ague. However, he put him under chloroform, and no bad result followed. In a case of urinary fever he would put the patient to bed, give him full doses of quinine, with morphia suppositories, aconite, perhaps a glass of hot grog, and in every way keep up the heat.

Dr. Patterson said that in passing a catheter in the case of a man in perfect health, the person would suddenly turn pale, and would ask to be allowed to sit down. That was shock. The effect of catheterisation in inducing a fit of ague in a person who has had the fever formerly was known to old soldiers. In one case under his care the veteran insisted on having quinine before the instrumentation, and on being asked his reason, replied at once that he had had ague in India. A good number of years ago he was asked by a student to see his companion, who was represented as suffering from abscesses in the back. The man was about 21 years of age, pale, pulse 120; voice weak; and below the angle of the left scapula was an abscess which, on enquiry, he found to be the growth of less than two weeks. As chronic abscesses did not form so rapidly, he was led to examine further; he found that this was only one of several abscesses. He had been suffering from chronic gonorrhœa, and the student had tried to cure him by passing a bougie, thus setting up this septic condition. In a hospital case some years ago he found a patient suffering from pyæmia. On examining the urethra he found a false passage filled with pus. In bladder cases there was considerable liability to septicæmia. In some cases, such as one he saw in Dr. Finlayson's ward, where the urine was large in quantity, pale in colour, and of low specific gravity, urea was retained in the blood. Then, if the kidney was not structurally affected, its function might be so deranged as to endanger the patient's life. In many cases of enlarged prostate the disease had been going on for years, and the bladder had never once been rightly emptied. It became thickened, its coats trabeculated, and the sudden emptying of its contents might lead to hæmorrhage. In many cases the patient was dying of slow poison from the urine retained in the bladder. All these circumstances tended to increase the risk in instrumentation in these cases. Cases of urethral fever were not so common in Glasgow now as they used to be.

Dr. Morton said that the discussion had somewhat diverged from the question of urethral fever. Dr. Cameron, no doubt did mention cases in which there was a tendency to pyæmia or septicæmia, but he did so only for the obvious purpose of eliminating such cases from the discussion. In these cases which had been spoken of as fatal, the men died from other causes. In many of these cases there was not simple stricture, but strictures complicated with false passages and other affections, and there was thus a tendency to septicæmia. But in a case of simple stricture, were they likely to meet with urethral fever? He said they were, and he was always prepared for it. His hospital assistant had a standing order to prepare and examine the patient carefully for the instrumentation. It was doubtful whether the term "stricture fever" was not preferable to that of urethral fever, as the fever might be regarded as a concomitant or a consequence of that condition of body which predisposed to or was caused by stricture. In many cases, chiefly of operations near the pelvis, in which after operation they had to pass the catheter, they had no stricture and no urethral fever. He was afraid that the explanation of the origin of such phenomena as occurring through the nervous system was merely a nominal explanation. It did not add to their knowledge of the phenomena. It was noteworthy that, though occasionally, after lithotomy, if the case went wrong there was this kind of fever, no such result followed the operation for chronic cystitis.

After a few remarks by Dr. Menzies—

Dr. Macewen said that he believed that had the patients mentioned by Dr. Cameron as having died of shock been placed under chloroform, the fatal result would have been averted. At the same time, had the hernia patient mentioned by him been anæsthetised, and the same result followed, it was probable that it would have been laid at the door of the chloroform. Dr. Macewen's subsequent remarks had reference to the theory which he had heard lately put forward by Drs. Finlayson and Cameron to account for hæmorrhage after catheterisation—viz., that when there was a great accumulation of urine in the bladder, the pressure therefrom gave a certain amount of support to the vessels, with the result of some of the vessels giving way when that support was withdrawn. This theory Dr. Macewen criticised. In regard to the resemblance of the symptoms of catheter fever to ague, he had a case lately in which the symptoms were like those of ague. There was no recent catheterisation, but an abscess had formed far down the urethra, and the bladder was found enormously distended. Since he had served under Dr. Macleod as house-surgeon he had always been very careful to carry out that gentleman's principles and practice as to making the most scrupulous preparation for instrumentation. Even on these being made he never liked to pass an instrument unless forced to do so. The quinine and opium treatment he had found very valuable.

Dr. Coats said that from the pathological standpoint he had been particularly pleased with Dr. Cameron's opening of the discussion as to the way in which he had discriminated different pathological conditions confounded under one general term. The pathology of the condition to which the name urethral fever was more properly limited, was very obscure. It was a condition supervening on some interference with the urethra, manifesting itself by rigors, suppression of urine, and other symptoms. The usual explanation given was, that there was set up nervous irritation, extending from the urethra to the kidney, in which organ a state of inflammation resulted. But it was difficult to see how a sudden stoppage of urine could be brought about in this way. No inflammation, he was aware, produced absolute stoppage of urine. The simple inflammatory theory therefore appeared to be untenable. What was the meaning of these rigors? To what was to be attributed the extreme pallor of the face to which Dr. Patterson had referred? It had occurred to him that as the rigors at the onset of acute fevers were due to spasms of the arteries of the skin, that on a similar principle might be explained the suppression of urine. If there was a violent irritation of the urethra it would cause stimulation of the vaso-motor centres, and thus we might have such a contraction of the arteries of the kidney as to produce a stoppage of the circulation of the kidney, and consequent stoppage of the secretion.

Dr. Cameron, in reply, said that the discussion had diverged into channels which he purposely warned the members from allowing it to enter—viz., into considering affections which, though confounded with, were not urethral fever. To one point he would advert. Some speakers had dwelt on the fact that in these cases there was generally a state of disease of the kidney. Now, in the case of Mr. Banks, which he had read, there was no affection of the kidney. In the case related by Sir Joseph Lister the kidney was perfectly healthy. He was of opinion that the mischief was really due, not to previous disease, but to nervous shock. As regarded the determination of an attack of ague, in persons who had previously suffered from it, by surgical operations, this had been remarked on by Sir James Paget and others. In fact, the infliction of an injury, apart from surgical interference, might have the same effect. As respected the treatment of this fever he had purposely avoided the full consideration of it, in order to bring his opening statement within due limits. He believed that this discussion would not be in vain if it contributed to producing a greater respect for the catheter.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

DISCUSSION ON ALBUMINURIA, ITS PATHOLOGY AND CLINICAL SIGNIFICANCE.

Session 1883-84.

Meeting VI.—12*th February*, 1884.

The President, Prof. M'Call Anderson, *in the Chair.*

The Pathology of Albuminuria in its Relation to Morbid Structural Changes in the Kidney.

By DAVID NEWMAN, M.D.

Mr. President and Gentlemen,—In the brief time at my disposal this evening it is not possible for me to discuss the whole pathology of albuminuria. I shall, therefore, content myself by bringing under your consideration one or two points in connection with the relationship of albuminuria to morbid structural changes in the kidney. The questions which present themselves most prominently for our consideration appear to me to be—

1. Why does the urine not normally contain albumen?
2. What are the renal changes associated with albuminuria, and how do they explain the absence of albumen from the urine in health, and its presence in disease of the kidney?

In discussing these questions, I shall exclude all cases where albuminuria arises from the presence of blood and pus, or where the albumen is derived from other parts of the urinary apparatus than the kidneys; also those cases where the urine is temporarily albuminous as a result of some abnormal condition of the blood or other causes, such as hepatic and alimentary albuminuria, &c. In short, I shall limit my remarks to those cases in which the urine has become albuminous as a result of disease of the kidneys.

Before I venture to discuss these questions, perhaps you will permit me to say a word or two regarding the normal structure and functions of the kidneys, as there are some points in connection with their anatomy and physiology to which I desire specially to direct your attention. I trust, therefore, that you will pardon me for introducing matter which, at first sight, may appear to be foreign to the discussion.

As regards its histology, the kidney may be said to be composed of three elements—(1) the blood-vessels; (2) the tubuli uriniferi; and (3) the lymphatics. In its ultimate distribution the renal artery may be divided into three portions, corresponding to the medulla, cortex, and capsule. It is with the vessels of the cortex that we have most to do. The renal artery, as it passes into the substance of the kidney, sends by far the greater portion of its blood to the cortex. The interlobular arteries run straight from their origin towards the periphery, and in their course give off numerous branches, the *vasa afferentia,* which pass directly to the dilated extremities of the uriniferous tubules. After entering the capsules of the Malpighian bodies they split up into convoluted plexuses, the *glomeruli;* they then gather together to form efferent vessels, which are much smaller in their calibre than the afferent arteries, so that the blood pressure is greater in the glomeruli than in the capillary circulation. The convoluted plexus of arteries which forms the glomerulus cannot be regarded as a true capillaries system; the proper capillary system being on the distal side of the Malpighian body. After leaving the Malpighian body the efferent vessel divides into two portions—(1) the wide meshed capillary network of the medullary rays; and (2) a network of small capillaries distributed to the tortuous tubules which afterwards go to form the *venæ stellatæ* and the interlobular veins. The blood supplying the medullary portion is derived almost exclusively from the *arteriolæ rectæ,* which enter the medulla from the side of the cortex, but the medulla also receives a little blood from the *vasa efferentia* of the Malpighian bodies situated close to the medulla. The arterial fasciculi formed of the *arteriolæ rectæ* are distributed to the tubules of the medulla, and after forming a capillary plexus, they join together to form the *venulæ rectæ.* These unite with the interlobular veins, and form the radicles of the renal vein. The two points to which I wish to call attention are—(1) the very direct manner in which the blood is conveyed to the glomerulus; and (2) the small size of the efferent vessel, and the extensive capillary and venous systems through which the blood has to pass after leaving the Malpighian body before it reaches the renal vein. To use the description of Mr. Bowman—"It would be difficult to conceive a disposition of parts more calculated to favour the escape of water from the blood than that of the Malpighian body. A large artery breaks up in a very direct manner into a number of minute branches, each of which suddenly opens up into an assemblage of vessels of a far greater aggregate capacity than itself, and from which there is one narrow exit. . . . Hence must arise a very abrupt retardation to the velocity of the current of blood." Besides the arrangement by which a large volume of blood is exposed to circumstances most conducive to free filtration of its fluid constituents, we have a condition, namely, the presence of a secondary capillary system on the distal side of the glomerulus, which, by its resistance to the onward flow of the blood, subjects the blood inside the Malpighian body to considerable pressure. In the cortical portion of the kidney the tubuli uriniferi end in globular dilatations (the capsules of the Malpighian bodies), enclosing the convoluted coils of vessels. The uriniferous tubule, on leaving the Malpighian body, becomes constricted so as to form the neck of the capsule. It then dilates and continues of about the same calibre through the tortuous portion, after which it becomes constricted as it passes towards the medullary region to form the loop of Henle; the descending portion of the loop being smaller than the ascending. The ascending limb, on passing into the intermediary portion, dilates and convolutes, and again becomes constricted before it enters the collecting tubes. These tubes unite with the others of the same pyramid to form a collecting tube which passes downwards, and with its fellows constitutes a papillary duct. The medullary regions are therefore made up of the narrow loops of Henle and the collecting tubes. The lymphatics of the kidney are most abundant in the cortical substance, particularly in the fibrous capsule, where they form a superficial plexus. The lymphatic network of the capsule is in free communication with the deep lymphatics which issue from the hilus, and around the tubules and capillaries there are freely intercommunicating spaces. In a healthy kidney the lymphatics may be injected by forcing fluid into the spaces in the capsule; and in the other room you may see a specimen prepared by my assistant, Mr. H. Lyon Smith, where they are injected from the ureter.* By a research which I conducted some time ago, I learned that in desquamative nephritis the lymphatic spaces are unusually large, while in interstitial nephritis they are almost entirely obliterated by inflammatory products. To this point I shall refer more fully hereafter.

In reference to the theories held regarding the method by which the urine is excreted, it is now generally supposed that

* This specimen is described by Mr. Smith in the Report of meeting, page 219.

the separation of urine takes place by filtration of a dilute solution of the constituents of the urine through the glomerulus into the capsule of the Malpighian body. This weak solution then passes along the tubuli uriniferi, where it comes in close contact with the blood it has just left. It is supposed that an interchange then takes place between the blood in the capillaries surrounding the tubules and the fluid inside, by which a certain amount of the water passes again into the blood, and so leaves the urine in a more concentrated state than it was when it first passed from the glomeruli into the dilated end of the uriniferous tubules. It is believed, however, that the epithelium which lines the convoluted tubes performs certain functions in connection with the secretion of the solid constituents.

The influence of nerves upon the secretion of urine has been demonstrated by experiment. It has been shown, for instance, that section of the renal, or of the splanchnic nerves, produces an increase in the secretion, and section of the cord below the medulla leads to a great general vascular dilatation, a consequent fall in the blood pressure in the glomeruli, and a diminution in the quantity of urine secreted. Stimulation of the cord below the medulla, although producing exactly the opposite result—viz., general vascular constriction, does not cause an increase in the flow of urine so long as the nerves of the kidney are in action, but if they be divided, the walls of the renal arteries become relaxed, the blood pressure in the glomeruli and the volume of blood passing through the kidney are consequently increased, and a copious flow of urine is the result.

It is, therefore, evident that constriction of the arteries, or arterioles of the body, so long as the vessels of the kidney are not involved, causes an increase in the flow of urine, but if they also be contracted, the pressure of the blood in the glomeruli will probably be reduced, and the secretion of urine diminished.

Viewed from a pathological standpoint, this fact is of importance. In Bright's disease, as you are aware, some observers have explained the copious flow of urine in interstitial nephritis by the increased arterial tension associated with it. The high tension results from hypertrophy of the heart, and of the muscular coats of the arteries, not only of the kidney, but also of other parts of the body. Now, that a copious flow of urine is partly the result of an increase in tension of the glomeruli, I do not deny, but I cannot admit that the high tension in the glomeruli in such cases is dependant upon the general high arterial tension arising from what has been called arterio-capillary fibrosis. For unless, as we have seen, the resistance to the entrance of blood into the renal arteries is less than that to the other arteries of the body, there can be no increase in the tension of the blood in the glomeruli; and in arterio-capillary fibrosis the thickening of the arterial walls is as great, if not greater, in the arterioles of the kidney than elsewhere. If, however, the hypertrophy of the heart more than compensates for the increased resistance in the renal arteries, or if the general arterial constriction be greater than that of the renal arteries, it is evident that the blood pressure in the glomeruli will rise, and an increase in the flow of urine will follow. This, I should say, is a rare circumstance in cases of renal disease. It may be said that in the contracted kidney a large number of the glomeruli are obliterated, and that, therefore, although the renal artery may be reduced in calibre, the blood supply is proportionally greater than normal; but in reply to such an argument, I have only to remind you that there are evidences of high tension in the glomeruli before there is reason to suppose that there is any obstruction in the Malpighian bodies themselves, or in the vessels on the proximal side of them.

The fact that the secretion of urine is partly due to a process of filtration under pressure is not now a subject of dispute, it is the composition of the filtrate that is still a matter of doubt.

Thus Bowman adopts the view that the glomeruli simply separate the water, the secretion of the specific products of the urine being the special function of the cells of the uriniferous tubules; while the theory advanced by Ludwig is, that the serum of the blood, less the fats and albuminates, filters through the walls of the glomerulus into the capsule of the Malpighian body, the filtrate then passes along the convoluted tubules, and comes in contact with the epithelium and the lymph contained in the lymphatic spaces surrounding the tubules; and a part of the water of this dilute filtrate is then re-absorbed. This is believed to take place by a process of diffusion between the fluid in the tubuli uriniferi and that contained in the lymphatic spaces and veins surrounding the tubes on all sides.

I do not consider that either of these theories explains the facts we have before us.

By the anatomical arrangement of the glomeruli we have a comparatively large afferent arteriole breaking up to form a number of loops of a greater aggregate capacity than itself, which collect together to form an efferent vessel of much smaller lumen than the afferent arteriole; and, besides, there is the efferent resistance of the true capillary system on the distal side of the glomerulus. The blood in the inside of the glomerulus is therefore subjected to considerable pressure. Now, if we apply the same conditions of pressure to any of the other capillaries of the body, what is the result? If the portal vein be obstructed the fluid which escapes does not contain only crystalloids, but it is also rich in albumen, the percentage of albumen being in most cases in proportion to the amount of portal obstruction. In some cases the proportion of albumen is very small, while in others the ascitic fluid approaches in composition the serum of the blood.

As far as I am aware, there have been no experiments performed to prove anything like conclusively the composition of the urine as it passes into the first part of the uriniferous tubules; but, notwithstanding, it has been assumed that albumen does not pass through the glomerulus of the normal kidney, on account of some peculiarity in the structure of the Malpighian body. Now I would like to know what there is in the structure of the Malpighian body to prevent the passage of albumen into the first part of the convoluted tubules.

Some portions of the urinary tract have undoubtedly the power of absorbing albumen. For instance, I have found, by experiment, and I believe my friend Dr. Finlayson has satisfied himself clinically, that the urinary bladder has the power of absorbing albumen. May it not be possible that in the normal condition a certain amount of albumen does pass from the glomeruli into the first part of the tubuli uriniferi, but that, under ordinary circumstances, when the kidney is healthy, the albumen is re-absorbed in some part of its course between the Malpighian bodies and the pelvis of the kidney? In cases where the tubular epithelium is diseased, where the amount of urea in the blood is augmented, and the arterial tension increased, not only will the tendency to the passage of albumen be greater, but the epithelium will no longer be in a condition to re-absorb it. This would explain why it is that globulin, which is often found in albuminous urine, is not found in healthy urine, although it is well known that, unlike serum albumen, it can diffuse itself through animal membranes with great ease. We must, however, admit that the appearance of albumen in the urine is to be explained by some alteration from the normal processes by which the urine is secreted. It must also be remembered that in some cases of disease of the kidney where there are pathological changes in their structure, even of an extreme nature, no albumen may appear in the urine; and again, in cases where no change can be detected, the urine may be highly albuminous.

The rapidity of the secretion of urine may be said to depend upon the following factors:—(1.) The quantity of urine excreted is in proportion to the difference that exists between the pressure inside the glomeruli and the tension of the urine in the Malpighian capsule and uriniferous tubules. (2.) The rapidity of the circulation of blood through the kidney, which depends upon the relationship between the afferent (arterial) pressure, the efferent (venous) resistance, and the tension of the fluid in the uriniferous tubules. (3.) The activity of the lymphatics. (4.) The quality of the blood in the artery of the Malpighian tuft. (5.) The state of the walls of the artery, constituting the Malpighian tuft, and of the capsule itself, these being regarded as the filter through which the water and constituents of the blood serum have to pass. (6.) The activity of tubular epithelium.

To illustrate the pathology of renal albuminuria, I shall now describe two typical cases, one of sub-acute parenchymatous, the other of moderately advanced chronic interstitial nephritis; and then I shall give you my replies to the questions which I proposed at the beginning of my address.

I shall only give you a brief summary of the clinical history and *post mortem* appearances in these two cases.

The first case was one of chronic interstitial nephritis. The patient, aged 48, died from pneumonia. The following is the history of the symptoms referrible to the kidney disease, from which he suffered for four years:—Polyuria (90-110 oz.), urine, pale clear; sp. gr., 1010-1012; albumen present, but small in amount, varying from ·2 per cent to a trace. Average, in ten observations made during the last two months of life, ·12 per cent; hyaline and granular tube casts detected on several occasions, but never very abundant. Patient suffered from shortness of breath and dyspeptic symptoms, and gradual failure of strength; body emaciated slowly, but there was no œdema till within ten days of death. Urine suddenly became scanty, but still retained its low sp. gr. 48 hours before death. Patient at this time fell into a low wandering delirium, followed by a fit of convulsions, from which he died.

The *post mortem* revealed pneumonia of lower lobes of both lungs, slight pericardial effusion, hypertrophy of left ventricle without valvular disease, early and limited cirrhosis of the liver, and the following appearances in the kidneys:—Both kidneys reduced in size and weight; right $4\frac{1}{2}$ oz.; left 4 oz. Capsules dense, thickened, and firmly adherent to the surfaces, which were granular and marked by cicatrices. The renal artery of the right kidney was injected by a continuous pressure, equal to 65 mm., and the left with a pressure equal to 100 mm. of mercury.

In the right kidney the glomeruli of the unaltered Malpighian bodies were well injected; but where the capsules had become much altered the injection was either very partial or the loops were entirely uninjected. The capillaries were not well injected except at a few places. There was no escape of fluid into the tubules or into the lymphatic spaces, and the injection of the lymphatics from the capsular lymphatic spaces was very unsatisfactory.

In injecting the left kidney more force was employed; but the injection of the vessels was not more complete: there was, however, considerable effusion of the gelatine mass into and around the convoluted tubules. The lumen of the arteries was reduced, partly as a result of hypertrophy of the muscular coat, and partly from an increase in the thickness of the internal coat.

On making a section parallel to the surface of the kidney the centres of the granulations were seen to correspond to the situation of the collecting tubes. The epithelium of these tubes was clear, their lumen normal, except in a few instances where they are obstructed by hyaline or finely granular tube casts. In the periphery of the granulations —namely, the part occupied by convoluted tubules and Malpighian bodies, there was atrophy of the normal elements, and a hyperplasia of the connective tissue in various stages of development. In the convoluted tubules the epithelium cells were small, round, more or less granular, and at some places separated from the basement membrane, which had become blended with the surrounding connective tissue and infiltrated with opaque granules. On making a section at right angles to the surface the cortex was seen to be reduced in thickness, the blood-vessels and uriniferous tubules tortuous and distorted, and the loops of Henle dilated, and in some places forming cysts. The efferent canals were almost normal. The capsules of the Malpighian bodies were surrounded by concentric zones of connective tissue, of varying thickness and density. In some parts the glomeruli were not greatly altered, and were well distended by the injecting mass; whereas, in other parts, particularly immediately under the capsule, the vascular loops were united together by connective tissue elements, the glomeruli atrophied, distorted, and in some instances only represented by little spheres of firm connective tissue.

In the case of parenchymatous nephritis we have a very different series of symptoms and *post mortem* appearances.

In the one which I am now about to describe the patient was young, 18 years old, and had been exposed to damp and cold during a whole night. The invasion of the disease was abrupt, and the first symptoms were shivering, headache, vomiting, &c., accompanied by general anasarca and slight effusion into the serous cavities, urine scanty, specific gravity 1018-1029, turbid, high coloured, albumen from 2·1 per cent to 4·3 per cent, average 2·8 per cent. Urea average of ten observations, 1·1 per cent. Abundant deposit of renal epithelium and blood, epithelial, and granular tube casts.

The patient had no uræmic symptoms, and the urine, which amounted at first to a few ounces a day, increased by the fourteenth day of the disease to 30 ounces; after that time she took a severe attack of diarrhœa, and died on the sixteenth day. At the *post mortem* examination the following conditions were found—general anasarca, slight serous effusion into both pleuræ, hepatisation of lower lobe of left lung, serous fluid in pericardium, heart practically normal, liver fatty.

The kidneys were increased in size, surface smooth, capsule thin and easily separated; on section the cortex was found to be very voluminous, pale in colour, opaque, soft and doughy, and the Malpighian bodies prominent. The medulla was traversed by opaque lines. Microscopic examination showed the lesion to be practically limited to the convoluted, and a few of the straight tubules. The convoluted tubules were opaque, and some of them dilated, so that their external measurement was larger than normal, while in nearly all the lumen of the tubules was reduced. The epithelium of the convoluted tubules and the ascending loops of Henle was granular, and a considerable number of the tubules contained tube casts. There was no increase in the intertubular connective tissue. The glomeruli were clear or slightly opaque, and most of them well filled with the injected fluid; the capillaries were also injected but not well. A quantity of the gelatine escaped into the lymphatic spaces around the tubules, and in some instances the Malpighian capsules also contained some of the mass, even though the glomerulus was not itself injected. The lymphatic spaces in the extreme cortex were easily injected in the right kidney. The vessels in the left kidney were injected with a pressure of 45 mm. of mercury. The capsules of the Malpighian bodies were not thickened, but between them and the surface of the vascular loops in a few instances there was a multiplication of the endothelial cells.

If we now contrast the symptoms and *post mortem* appearances in these two cases, it will be seen that the prominent points of distinction are—1. In *chronic interstitial nephritis* —Urine abundant, slightly albuminous, œdema not an early symptom, evolution gradual, kidneys small, capillaries not easily injected, fluid does not pass into lymphatics or tubules unless under high pressure, glomeruli well injected, disease limited at early stage to intertubular connective tissue, afterwards epithelium of cortical tubules, atrophy, and the straight tubules are only affected after a long time, Malpighian capsule thickened, and glomeruli atrophied or thickened, capillary resistance increased.

2. In *acute parenchymatous nephritis*, the course of the disease is rapid, urine scanty, highly albuminous, œdema an early symptom, evolution relatively rapid, kidneys enlarged, capillaries and glomeruli easily injected, and fluid escaped into lymphatic spaces under low pressure, lesion practically limited to epithelium, no hyperplasia of connective tissue, or thickening of Malpighian capsules, arteries normal, capillary resistance increased.

Now, the questions which naturally suggest themselves in connection with these two diseases are, Why, in interstitial nephritis, is the quantity of urine so large, and not only the percentage but also the absolute quantity of albumen lost in twenty-four hours so small, whereas, in parenchymatous nephritis the secretion of urine is small, but the percentage and absolute quantity of albumen lost is great.

Undoubtedly, in both diseases the capillary resistance is greatly increased, in the one case by compression of the capillaries from the pressure of the over distended uriniferous tubules, in the other from the increase in and contraction of the connective tissue. The amount of blood, however, which passes to the kidney in each diseased condition is very different; for while in interstitial nephritis the lumen of the renal arteries and afferent arterioles is greatly reduced from endarteritis obliterans, in the more acute disease these vessels remain practically normal. Although in chronic interstitial nephritis the *general* arterial tension may be raised considerably by the hypertrophy of the left ventricle and the so-called arterio-capillary fibrosis, it is evident that the narrowing of the vessels on the proximal side of the glomeruli will tend to diminish the amount of blood passing to the Malpighian bodies and to reduce its tension. So that if the tension of the glomeruli is increased, it is not from the conditions which lead to the general high arterial tension, but is the effect of the increased capillary and venous resistance on the distal side of the Malpighian body.

This increase of the efferent resistance, as I have already shown, is also present in acute parenchymatous nephritis, so that polyuria cannot be explained by it alone. In both diseases the quantity of fluid filtered through the glomeruli is increased.

What other conditions, then, have we in the chronic disease to explain it? The thickening of the capsules of the Malpighian bodies would tend rather to diminish than to increase the amount of fluid filtered through the glomerulus, and the destruction of a certain number of the Malpighian bodies would have the same effect. We must look for the explanation elsewhere; the condition of the filter does not seem to explain the polyuria. In speaking of the physiology of the secretion of urine, I said that probably the serum of the blood filters through the walls of the glomerulus in a greatly diluted state, and that, as it passes along the uriniferous tubules it comes in contact with the lymph in the lymphatic spaces, the epithelium and the capillary network surrounding the tubules; a part of the water of this dilute filtrate is re-absorbed.

In the blood-vessels and tubules we have two distinct kinds of fluids—in the former an alkaline (the blood), in the latter an acid (the urine). Now, it is well known that alkaline solutions produce endosmosis to a remarkable extent, a large quantity of water passing to the side of the alkalies (positive osmosis), and that the equivalent increases with the degree of concentration. On the other hand, dilute acids and solutions of acid salts produce a current in the opposite direction (negative osmosis), and the equivalent diminishes as the concentration increases. Therefore, to induce osmosis under the most favourable circumstances, we should have an acid solution on one side of the membrane and an alkaline on the other. But if, in addition to the difference in kind, we have a difference in degree of concentration, the alkaline being the more concentrated of the two, then we have the conditions most advantageous. This is exactly the condition we have in the arrangements of the kidney; we should, therefore, expect positive osmosis to exist in the circulatory system, and negative osmosis in the urinary.

Now, the rate at which endosmosis takes place in a diseased kidney will depend upon the condition of the lymphatics and the proximity of the blood-vessels to the uriniferous tubules.

In interstitial nephritis, as I have already shown, the lymphatics are greatly obliterated by inflammatory products, and the capillaries are separated from the tubules by layers of connective tissue, whereas in parenchymatous nephritis the lymphatic spaces are large, the epithelium has desquamated, and as a result the capillaries are in close contact with the uriniferous tubules: in the latter disease, therefore, the conditions are favourable to rapid osmosis from the urinary to the vascular and lymphatic systems, whereas, in the former they are not, and therefore a large quantity of dilute urine is excreted; in other words, the patient suffers from polyuria.

The smallness in the quantity of urine in acute parenchymatous nephritis is not altogether due to the increased facilities for re-absorption by osmosis, but probably is also a result of obstruction to the passage of urine along the uriniferous tubules or collecting tubes by masses of desquamated epithelium, blood clots, or tube casts.

I now come to the consideration of the relative quantities of albumen lost in the two diseases of which I have been speaking. In both the albuminuria has a close and definite relationship to the histological changes in the kidney.

In acute parenchymatous nephritis the quantity of albumen lost is large, and may be derived from several sources.

1. It may be the result of over distension of the Malpighian bodies from an increase in the amount of blood conveyed to them, or from an undue resistance in the capillaries and veins. The passage of albumen may also be facilitated by a diminution in the resisting power of the glomeruli. By reason of this increased distension a larger proportion of the albuminous constituents of the blood serum are permitted to filter through the glomeruli.

2. The function of the epithelium of the uriniferous tubules being interfered with by degenerative and inflammatory changes, a portion of the albumen which has escaped through the glomeruli is allowed to pass and appears in the urine.

3. From the fact that parenchymatous nephritis is an acute inflammatory condition associated with rapid proliferation and desquamation of the renal epithelium, it is not unlikely that, as in other catarrhal affections, a certain amount of albumen is

derived from inflammatory exudation into the uriniferous tubules.

In chronic interstitial nephritis, as I have already pointed out, the quantity of blood circulating in the kidney is diminished, but, notwithstanding, on account of increased capillary and venous resistance the tension of the blood in the glomeruli is raised. Now, the effects of this increase in the glomerular tension are twofold: (1) it causes a relative increase in the amount of urine excreted, and (2) by a stretching of the structures forming the glomeruli the openings or passages through which the fluid filters are widened, so that the filtrate has a closer relationship to the blood serum than normal. A larger quantity of albumen therefore escapes, and the function of the epithelium not being active enough to take it all up, a portion of it appears in the urine.

The albuminuria of chronic interstitial nephritis has therefore a much simpler explanation than albuminuria arising from acute diseases of the kidney.

I may now answer very shortly the questions which I proposed at the beginning of my address. The first was: Why does the urine not normally contain albumen? And my answer to it is, Because the albumen is re-absorbed by the renal epithelium as it passes along the uriniferous tubules and not as a result of any special function of the glomeruli.

The second question is more difficult to answer shortly, but, from what I have already said, I have no doubt that you anticipate my answer—namely, That the principal causes of albuminuria are: (1) increased tension of the blood in the glomeruli from whatever cause it may arise, whether from increase in the afferent pressure or from undue efferent (capillary or venous) resistance; (2) inflammatory and degenerative changes in the renal epithelium. The first causes an increase in the escape of albumen from the blood, and the second interferes with its proper absorption by the uriniferous tubules.

Inflammatory exudation may escape into the tubules either from their own walls, or from Bowman's capsule when the Malpighian bodies are the seat of acute inflammation.

Physiological Albuminuria, Globulin-Reaction, and Tests for Albumen.

By Dr. WILLIAM ROBERTS, of Manchester.

Mr. President and Gentlemen,—My first duty is to thank you for the compliment you have paid me in inviting me, a physician from across the border, to take part in this important discussion.

The subject of albuminuria, in its clinical and pathological aspects, is so wide, that it would scarcely be possible for one speaker to deal with it, in its various branches, within the limits necessarily imposed in a discussion like the present. The council of the Society, or those who are responsible for the conduct of this debate, have wisely recognised this limitation, and have entrusted to Dr. Newman, by way of laying a sound foundation, the task of considering the subject in its relations to the physiological and pathological histology of the kidney. But, even after making abstraction of this portion of the subject, there remains so large a field in the purely clinical relations of albuminuria, that I shrink from the attempt to deal with it in its entirety. I should, I fear, only lose myself in vague generalities were I to take that course; and I judge it better to confine myself to certain points, or divisions, of the subject, in regard to which I may have some fresh information to communicate, or some useful suggestions to make. Albuminuria, as encountered in the hospital ward and the sick room, is usually associated with some grave disorder—and often with incurable organic disease. We find it associated with structural changes in the kidneys, in the various forms of Bright's disease, and in the less serious condition of renal catarrh. We also meet with it as a frequent occurrence in diseases which involve a disturbance in the circulation of the blood—especially in affections of the heart and lungs. The appearance of albumen in the urine is a frequent incident in febrile and inflammatory complaints of all kinds—and we see it as one of the terminal phenomena in the later periods of all sorts of fatally tending diseases.

But we also meet with albuminuria in perfectly healthy, or substantially healthy, persons. For the most part, these cases are stumbled on accidentally and unexpectedly—perhaps in examining a candidate for life insurance; or perhaps in the routine of the consulting room in testing the urine of a patient; or perhaps in the course of an experimental inquiry into the urines of a group of persons supposed to be in the highest health, such as boys at school, a class of students, or a company of soldiers.

This class of cases is still but imperfectly understood and known, and it seems desirable to attract more attention to them, with a view of having them better observed and better defined. As the matter stands, I apprehend that a case of this class is, to most of us, either a mere puzzle, or a source of serious anxiety and embarrassment in the way of diagnosis and prognosis. Moreover, it is not improbable that a study of these simple, elemental, quasi-physiological examples of albuminuria may throw important light on some of the difficult problems which confront us in the study of the graver forms of albuminuria. I propose, therefore, to devote the time at my disposal partly to a consideration of this class of cases, and the remainder to an examination of the methods, both new and old, of testing for albumen in the urine.

But before entering on these subjects, I should like to advert to two points in connection with the albuminuria of Bright's disease, and more by way of suggestion, and as seeking information, than as communicating anything new.

Globulin reaction.—The first of these points is this—Some of you, I daresay, have noticed the peculiar reaction which is sometimes seen when an albuminous urine is largely diluted with water. If you fill a urine glass, or a large test tube, with water, and let fall into it a succession of drops of an albuminous urine from a case of Bright's disease, you will see, in many cases, that each drop as it sinks is followed by a milky train, and that when a sufficient number of drops has been added, the water assumes throughout an opalescent appearance, as if a few drops of milk had been added to it. The addition of a little acid causes the opalescence to disappear. This reaction appears to be due to the presence of globulin (or rather paraglobulin) in the urine. One of the characteristics of globulin is that it is insoluble in pure water, but dissolves in water containing a certain percentage of neutral salts in solution, or in water rendered a little acid or a little alkaline.

The rationale of the reaction seems to be this—Albuminous urines generally, if not always, contain globulin as well as serum albumen; and in many cases the proportion of globulin is very considerable. This substance is kept in solution in the urine by the sodium chloride, and other neutral salts, which are always present in the secretion. But when such urines are largely diluted with pure water—ordinary potable water is pure enough for this purpose—the percentage of neutral salts is so reduced that the globulin falls out of solution and forms a cloudy precipitate. I am not aware that any attention has been directed to this reaction, which, with some albuminous urines, is a very striking one, nor whether any information of a diagnostic or prognostic value can be drawn from it, but it might be worth inquiring into.

Retrocession of hypertrophy of the left ventricle in chronic albuminuria.—The other point to which I wish to refer is a question of prognosis. Until lately it has been with me, as I presume with others, an article of belief that when, in the course of chronic albuminuria, a pronounced enlargement of the left ventricle is slowly established, this enlargement is permanent, and that it indicates the existence of advanced chronic Bright's disease. But there has passed under my observation, within the last three years, a case in which a chronically established and highly pronounced hypertrophy of the left ventricle in an albuminuric patient disappeared entirely. The patient was a man of sixty, with a distinct, though slight, gouty tendency. When first seen he had indolent œdema of the feet and ankles, pale sallow complexion, albuminous urine of low density, and moderately abundant in quantity. There were no cardiac murmurs, but the heart was enlarged to a moderate degree. He was a spare man, and the heart's dimensions were easily measured with exactitude. I had unusual opportunities of watching the progress of events in this case. In the course of eight months the heart attained very considerable dimensions, and exhibited the labouring action and widely diffused impulse usually witnessed in cases of cirrhotic kidneys. I have, indeed, seldom seen a more characteristic example. Yet this man slowly recovered. The heart retreated within its normal limits, and the albumen finally disappeared from the urine. This series of events covered a period of twenty-three months. I should much like to know if a similar experience has fallen in the way of other observers.

Albuminuria of healthy persons.—I now pass on to consider the albuminuria of healthy persons, or, as it may be conveniently, and perhaps not altogether incorrectly, called, physiological albuminuria. The general features of this form of albuminuria have been made known through the published observations of Moxon, Dukes, Saundby, Ultzmann, Leube, Runeberg and others. The condition is still but ill defined: and further information is much wanted. I have encountered a good many examples in my own practice; and guided by this experience and an examination of the published accounts, I may say that the typical cases answer roughly to the following description. The albumen is generally small in quantity, its occurrence is intermittent—it is generally absent in the morning urine, after the rest and fasting of the night, and present in the urine of the day; it goes and comes on successive days. The albuminuria is usually transitory—it lasts a few days, a few weeks, or a few months. As a rule, the urine contains no deposit of urinary derivatives—but sometimes a few narrow hyaline casts are discovered. It never contains blood even in microscopical quantity. The urine is quite normal in its other properties—in colour, density, volume, reaction, and in the relative proportion of its component ingredients. There is never any dropsy or other collateral sign of Bright's disease. The general health is often ideally perfect—more often the level of health is below par. There is complaint of being easily fatigued, of not eating as well as usual, there is pallor and a lack of muscular vigour. The subjects of this form of albuminuria are for the most part growing boys and girls, persons about the age of puberty, and young adults under the age of thirty. Some cases deviate in some particulars from this general type. The albuminuria may be constant throughout the twenty-four hours—it may last for years without the least declension of health—it may occur in middle age. In a very few cases the quantity of albumen is very considerable, and this large discharge may go on, as I have myself witnessed, for many months without the smallest detriment to health, and then slowly pass away. It has been said by some observers that the urine is apt to have a high specific gravity and a too acid reaction; but in the examples that have passed under my observation I have not noticed that the oscillations in density and reaction have at all exceeded those witnessed in healthy individuals.

What explanation can be offered to account for these cases? Is there really such a thing as a physiological albuminuria? I believe there is such a thing. It has been both affirmed and denied on high authority that traces of serum-albumen exist in normal urine; in other words, that there exists in health an insensible albuminuria. I have satisfied myself that concentrated urines from persons in undoubted health are comparatively rarely free from traces of albumen detectible by direct testing.

In the ordinary process of urine testing these traces are naturally overlooked, but they certainly exist very frequently, and their existence indicates how nearly on the verge of a sensible albuminuria healthy people are. This suggests the view that the so-called albuminuria of healthy persons may be a condition within the physiological range; and that it may not incorrectly be regarded as an exaggeration—abnormal certainly, but not morbid—of the insensible albuminuria of healthy life, something in the same way as the sweating which, in some persons, attends on mental emotion or muscular exertion, is an exaggeration of the insensible perspiration of the tranquil state. The standard of health is, as we know, not represented by a hard and fast line, but by a more or less ample range, through which the functions oscillate under the varying pressure of diverse influences both internal and external; and the amplitude of this range varies considerably in different individuals, and in the same individual at different times and ages.

The establishment of the view here suggested—namely, that there may exist a physiological albuminuria, which is but an exaggeration of the insensible albuminuria of ordinary health would, I think, prove an important help in the classification of cases of albuminuria. It would explain the extraordinary frequency in which albuminuria has been detected by observers who have examined the urine of a large number of individuals seriatim. For example, Leube found that 16 per cent out of 148 healthy soldiers examined by him passed albumen with the mid-day urine, after food and exercise; and that no less than 4 per cent had albumen in their morning urine. Dr. Saundby states that of 145 male patients taken seriatim, as they presented themselves in the out-patient department of the Birmingham General Hospital, 105 had albumen in the urine; and of these no fewer than 26 (18 per cent) could not be classified in the recognised categories of morbid albuminuria. It would also enable us better to group, define, and winnow the somewhat miscellaneous assemblage of cases which have been published under various headings—as "albuminuria of puberty," "albuminuria of adolescence," "intermittent albuminuria," "transitory albuminuria," &c.

Testing for albumen in the urine.—A little more than a year ago I published an account of a new test for albumen in the urine. This test consisted of a saturated solution of common salt acidulated with 1 per cent of hydrochloric acid. In the very same week Dr. George Johnson made public his observations on picric acid as a test for albumen. Shortly afterwards Dr. Oliver of Harrogate entered the field and gave us the results of a considerable amount of careful work on the various means of testing for albumen. He brought forward the tungstate test, which consists of a solution of tungstate of soda, strongly acidulated with citric acid. He also called attention to the potassio-mercuric-iodide test, proposed by C. Tanret of Paris. Dr. Oliver also introduced an ingenious method of rendering portable these tests by means of dried papers saturated with the test solutions. Considerable interest has in this way been excited in the profession on the subject of testing for albumen in the urine; and some unsettling of minds has been produced in regard to the use of the old established methods of testing by heat and nitric acid.

In the last few months I have gone over this ground with some care, and I propose to lay before you the results of my investigations. I soon found that the new tests possessed extreme sensitiveness; but I also found that they all had this serious drawback—namely, that they gave a reaction frequently with normal urines. With urines that contained a large or moderate amount of albumen they yielded a reaction which was quite unmistakeable; but with urines that contained only a small quantity, or only traces of albumen, their reactions required to be controlled by heat and nitric acid before they could be accepted as conclusive of the presence of albumen. I further found that the old tests, heat and nitric acid, but especially the heat test, possessed, when performed with certain precautions, which I shall point out presently, a delicacy, a certainty of action, and a freedom from fallacies; which rendered them distinctly superior to any of the new tests for ordinary clinical work.

What we are in search of in testing the urine is serum albumen: this and globulin are the two known morbid albumens of the urine. But other albuminoid or quasi-albuminoid bodies are present in the urine, both in health and in disease, such as peptones, hemi-albumose, and mucin. And it is evident that workable clinical tests for albumen should not give a reaction with these non-morbid albuminoids, but only with the morbid albumen. Against picric acid there are other objections which have been pointed out, but which I need not enter upon.

I may state generally, that any test for albumen which requires strong acidulation of the urine with an organic acid—such as citric, acetic, or lactic acid—is open to the objection that it throws down mucin—the essential constituent of mucus—which seems to be present in larger or smaller quantities in nearly all urines. All highly acidulated tests are for this reason unsuitable for the detection of minimal quantities of albumen in the urine. This objection throws out of court the tungstate, the mercuric-iodide, and the ferrocyanide of potassium tests—all of which require free acidulation to bring out their reactions.

The *mucin reaction* requires further investigation. I assume that the reaction in question is due to mucin, because I know of no other colloid substance which is thrown down in this way from normal urine by acetic, citric, and lactic acids except mucin. The neatest and surest method of bringing out the mucin reaction is by means of a concentrated solution of citric acid. If such a solution be added to urine in a test tube in the same way as in Heller's method of applying the nitric acid test—that is to say, allowed to trickle along the sides of the tube until it forms a distinct layer below the column of urine, you will see gradually forming an opalescent zone immediately above the layer of acid. Acetic and lactic acids are less appropriate for eliciting the mucin reaction than the strong citric acid solution, because, owing to their less specific gravity, they do not so readily sink to the bottom of the tube and form a distinct layer below the urine. But if acetic acid be mixed with one-third of its bulk of glycerine, it acquires the due density and answers perfectly as a mucin test. Sometimes mucin is so abundantly present that the free addition of acetic acid, without any precautions, produces a distinct milkiness in the urine. It is to be remarked that in such a case the urine may not exhibit any of the ordinary appearances of containing mucin, such as masses or flakes of glairy, stringy matter.

It is clear, then, to my mind that, in ordinary clinical work, we must keep to heat and nitric acid, and that the new tests may, for the present, be relegated to the laboratory. This brings me to the consideration of the best methods of applying the heat and nitric acid tests so as to bring out their maximum sensitiveness.

Heat test.—The results of my experiments indicate the following as the best method of performing the heat test. Ten cubic centimeters, or three fluid drachms, of the urine are placed in a long test tube of such bore that it will form a column of two or three inches. To this is added a single drop of acetic acid (B.P.). The upper half of the column is then heated to brisk boiling. If albumen be present, even in minimal quantity, the upper boiled portion of the column will show opalescence in contrast with the lower half, which remains unchanged. If the urine be alkaline, it should be carefully neutralised by adding successive drops of acetic acid until the litmus paper shows a faint acidity, and then the final single drop of acid is added before boiling. Even if the urine possess its natural acidity, and be turbid from lithates, it is better to add a drop of acid if you want to bring out the maximum sensitiveness of the boiling test. By using this small and definite quantity of acid you avoid provoking the mucin reaction—you also avoid, on the other hand, the risk of preventing the precipitation of the albumen by the use of too much acid. When the experiment is performed with these precautions, morbid albumen is thrown down to a certainty, and nothing but morbid albumen. I gauged the delicacy of this test in the following manner:—A moderately albuminous urine was diluted with 2,000 times its bulk of water. I could with certainty detect the presence of albumen in this dilution by the boiling test. Now the urine operated on was found, on a careful weighing analysis, to contain 0·76 per cent of dry albumen. If you work out the sum which these figures furnish, you get this astonishing result, that the boiling test, with due acidulation, enables you to detect albumen in a watery solution which contains only one part in 250,000! When a similar dilution of the same urine was made with a non-albuminous urine, instead of water, the sensitiveness of the test was found to be nearly as great.*

None of the new tests exhibits a sensitiveness equal to this. I found that the tungstate, the mercuric-iodide, and the picric acid tests gave a faint reaction in a watery dilution containing one part in 100,000. The nitric acid test also gave a reaction with this dilution after the lapse of, but not before, 30 minutes.

Nitric acid test.—With regard to this test, the only point I wish to call attention to is the importance of waiting a certain time for the development of the reaction. In the case of the boiling test you get the maximum reaction in 2 minutes at the latest; but with nitric acid you must wait 30 or 40 minutes before you exhaust the utmost delicacy of the test. The faint but distinct hazy zone, which is only fully developed after the lapse of 30 minutes after the addition of the acid, is just as certain a sign of the presence of albumen as a zone that develops immediately, or after the lapse of one minute. It is not often necessary to test with this extreme scrupulousness. In full coloured urines it is certainly not

* In searching for traces of albumen in a watery dilution of an albuminous urine, a much smaller quantity of acid must be added than in testing the urine directly. I know not the reason of this, but the watery dilution must not be acidulated with more than about $\frac{1}{10}$ of a drop per 10 cc., or the delicacy of the test is much impaired.

necessary; but in pale urines of low specific gravity the minutest trace of albumen is of most serious significance, and therefore worth looking for. Now the difference, in regard to the proportionate quantity of albumen indicated, between a reaction which is developed in 1 minute, and one which is developed in 30 minutes is, according to my experiments, in the ratio of abcut 4 to 1—that is to say, that by waiting for 30 or 40 minutes before accepting a negative conclusion, you increase the delicacy of your testing fourfold.

Pathology of Albuminuria and the Complications in Bright's Disease.

By Professor HAMILTON, Aberdeen.

I think, Sir, that all members of the Society will agree with me when I say that it has chosen for discussion this evening a subject which is at once one of the most complicated and most interesting in the whole range of pathologico-physiological science. For, at the very outset of such an inquiry, we are met with the strange and as yet unaccounted-for phenomenon, that in the natural secretion of urine the renal capillaries so far differ from those throughout the body generally, in the fact that they allow water and certain proper urinary constituents to pass through their walls, while the albumens contained in the blood are apparently unable to pass this delicate barrier. The systemic capillaries generally, on the other hand, so far as we know, are constantly transuding a fluid more or less loaded with serum albumen and with the other solid constituents of the blood. Here, to begin with, is a phenomenon which, until thoroughly comprehended, must continue to render the study of the subject of albuminuria more or less abortive. True, it has been asserted by Ludwig that the albumen of the blood does pass through the glomerular capillaries, but that it is re-absorbed by the lymph channels surrounding the tube farther down. Such a view of the functions of the glomerulus seems, however, to be now almost universally abandoned, there being several facts which seriously militate against it.

Then, again, it has been asserted that the layer of epithelium covering the glomerulus has the power of retaining the albumen and allowing the watery part of the blood to pass into the tubule. Such a theory presupposes that the albumen escapes from the capillaries, infiltrates the connective tissue or plasma spaces between the capillary loops of the glomerulus, but finds an effectual hindrance to its farther passage into the tube by the delicate layer of epithelium covering the loops. This theory must further presuppose that the albumen which has left the capillaries is re-absorbed or removed by the comparatively scanty lymphatic channels of the organ. Such a notion seems almost incomprehensible at first sight. Yet, in nature, there are several membranes which are possessed of very remarkable properties of this kind. Thus, the shell membrane of the fowl's egg will allow a solution of sugar to pass through it in one direction but not in another; the skin of the grape has a similar action; the gall bladder retains bile during life, and Descemet's membrane exerts a similar retentive action upon the aqueous humour. These are as yet unexplained physical phenomena, and a similar protective influence, so to speak, has been claimed for the epithelial covering of the glomerular loops in regard to the albuminous constituents of the blood.

Albuminuria, we all know (and as we have heard so clearly explained by Dr. Roberts this evening), may result from disease of the kidney, or it may ensue in cases where there is not any perceptible morbid condition of the organ. In the former case its causation is a very wide and complicated question, but in the latter it has been asserted that the retentive function of the renal epithelium has been impaired or lost, possibly for the time being, and that a certain proportion of serum-albumen is allowed to pass through. This idea I think every one will admit is highly theoretical, and would require much further experimental inquiry before it can be accepted. It seems, however, plausible enough as an explanation of those conditions of temporary albuminuria connected with eclampsia, excessive use of alcohol, and the intermittent albuminuria of pregnancy.

Turning now to another fertile source of albuminuria, namely, cardiac valvular disease, What theory can be advanced to explain this? Of course, the answer which will most readily be given will be that there is regurgitant venous pressure to an undue extent. The valve is insufficient, and a certain amount of the systolic energy of the left ventricle is expended, firstly, upon the lung, and, secondly, upon the whole right side of the circulation, acting of course very directly upon the renal veins on account of their direct connection with the vena cava. It has been urged, however, that any such increase of the positive pressure on the right side of the heart is just counterbalanced by the loss of propelling power in the left; that the energy exerted upon the blood mass is just equal to what it was before; so that any gain in positive pressure on the venous side is counterbalanced by loss of positive pressure on the arterial. Against this there is always to be borne in mind, however, that the ventricle is frequently hypertrophied, and that thus an additional propelling power may be absolutely gained. But it is urged by Cohnheim and many other German pathologists that neither section of the splanchnic nerves, nor division of the renal branches themselves, provided the renal blood-vessels are left uninjured, produces albuminuria. Further, that when the blood pressure is raised by simultaneous ligature of the abdominal aorta and the two subclavians, albuminuria does not result. That those who suffer from idiopathic boucardia, such as may be of nervous origin, so long as they are otherwise in good health, are not the subjects of albuminuria; and that where the kidney is cirrhotic and the left ventricle consequently hypertrophied, the quantity of albumen in the urine is extremely small. In all these cases there is supposed to be an increased arterial pressure. Why is it that in them the urine is not more constantly and highly albuminous? In regard to the experiment of ligaturing the aorta and subclavians, it is only right to state that Brunton has found albuminuria to occur in some cases but not in others.

It seems to me that it is almost impossible to consider these cases together, but that each must be argued on its own merits, and in terms of the factors present in each. It is extremely questionable, to begin with, whether what we call a solution of an albumen in water is in reality of the same nature as the solution of such a substance as a salt in the same liquid. Runeberg holds the view that it is not in reality a true solution or molecular mixture, but that the so-called solutions of albumen are, in reality, emulsions. Hence, if this be so, and physical experiments seem to bear it out, we have to do with a particulate substance suspended in a liquid. Now the albumens contained in lymph or dropsical fluids appear to be pure colloids—that is to say, they pass through a membrane only under pressure, and some very remarkable facts have been made out by Runeberg in regard to the circumstances which influence the amount capable of transuding. The membrane he experimented with was the intestine, prepared by being toughened in dilute alcohol, and the pressure employed was that of a mètre of water. When a fresh membrane is employed the relative amount of the albumen in the filtrate increases at first with the time until a certain point is reached, and after this it becomes constant. As soon as this constant stage is reached the albumen of the filtrate diminishes by raising, and increases by diminishing, the pressure. Further, a membrane which has been employed for the experiment and allowed to rest for some time, allows a larger quantity of albumen to pass than it did previously, when the pressure is re-applied. The explanation of these facts I hold to be that the intestine is a compound membrane, made up of several layers. Now, when pressure is first applied to such a membrane, it being very porous, and the pores being as yet free from any obstruction, a large quantity of a particulate substance, suspended in a liquid, such as we may suppose a solution of albumen to be, will pass readily through it. In course of time, however, the pores become clogged, and the passage of other particles is retarded. If, now, extra pressure is put on the membrane, the effect is that as the fibres composing it interlace in different directions in its various layers, the fibres of one layer are driven into the pores of the other, and an almost complete stoppage to the outflow of anything solid is produced. Whereas, if the pressure is diminished so as to relax the membrane, the porosity will become greater, because the pressure of the one lamina against the other will be diminished. Hence, one would naturally conclude from these experiments that, if in the kidney you diminish the blood pressure, provided that it has remained constant for some time previously, a greater quantity of solid matter ought to escape through the blood-vessels, and an albuminuria ought to result. There is an experiment which actually appears, at first sight, to tend towards this conclusion—namely (Overbeck), that if the circulation through the kidney be interrupted, only for a short time, by ligature of the artery, the urine subsequently shed contains albumen and blood corpuscles. Hermann made out that even if the renal artery be only partially ligatured, so as to limit the amount of blood entering the organ, an albuminuria is the result. These experiments are, however, utterly worthless, I hold, as an argument that diminished pressure produces an albuminuria. The usual explanation is, that the diminished supply of blood so influences the nutrition of the organ that the epithelial covering of the glomerulus becomes less resistant. I hold that there is quite another explanation of the hæmaturia and albuminuria following the above experiments, to be found in the results obtained by Litten—namely, that if the renal artery and vein be *both* ligatured, the organ, nevertheless, becomes so congested within a very short time that it may reach to almost double its natural size. Blood is effused into the tubes, and makes its appearance in the urine. The explanation is, that the anastomosis of the renal arteries with neighbouring branches is so free, that when the pressure is removed from the direct arterial stem it is almost immediately made up by an increased quantity of blood being forced in through the collateral channels. The circulation, however, does not go on so smoothly when the blood is supplied from such surrounding and unusual sources, and hence a congestion, with rupture and exudation of the blood contents into the tubes takes place. I, therefore, see nothing whatever in the experiments of Overbeck and Hermann to prove that diminished blood pressure in the kidney may be a direct cause of albuminuria, nor do I hold that Runeberg's results, accurate as they may be when a compound membrane like the intestine is under consideration, are apposite in explaining the phenomena of the filtration of albumen through a delicate simple membrane, such as exists in the endothelial wall of a capillary vessel. The two membranes are not to be compared, for while the one will resist very considerable pressure without exhibiting any sign of yielding, the slightest undue pressure on the other is sufficient not only to stretch it to an extreme degree, but, ultimately, to separate its endothelial plates, and to allow not a filtrate, but pure blood plasma to escape.

So far, therefore, as regards the albuminuria of valvular disease, I am strongly inclined to believe that it is due to over distension of the capillary vessels from regurgitant pressure. The whole appearance of the kidney favours this idea. It is deeply congested and much indurated. The capillary vessels of the tuft are distended to an unusual extent, and retain an extraordinary number of blood corpuscles after death. The interspaces of the tuft are distended as if from œdema, and the whole tuft is consequently enlarged. The straight vessels in the medulla are also engorged and dilated, and small ruptures with extravasations are here and there seen. Now all these appearances point, I hold, to there having been an unduly high venous pressure within the organ during life, the capillaries have been retained in a dilated condition, their walls have become more porous than normally from being stretched, while, on account of this undue porosity, an abnormally thick fluid, so to speak, has passed out—in some cases an actual rupture has occurred. It strikes me that the experiments made on this subject have been made too exclusively upon the renal *artery*. The attempt to raise the pressure in the artery by ligature of the aorta, or otherwise, is a very different thing from retarding the outflow of blood through the vein, for while the former operation is, as before mentioned, usually not accompanied by albuminuria, ligature of the vein gives rise, not only to albuminuria, but also to severe hæmorrhage.

In the cirrhotic kidney I am strongly inclined to believe that the cause of the albuminuria is the obstructed circulation within the organ. The kidney is often much congested. Some of the severest instances of congestion I have ever seen in the glomerulus have been in this disease. The capillary vessels and small veins are much compressed by the cicatricial tissue, the circulation is hindered within the organ, and from time to time an exacerbation occurs, in which the congestion becomes greater, and in which a larger quantity of albumen is exuded. As to why it is usually so small in amount, I think this may be accounted for by the chronicity of the disease—the circulation has time afforded to accommodate itself to the altered conditions of the organ to a certain extent, and hence an unduly great quantity of albumen is not forced out. The epithelium, no doubt, is never normal in this disease, and hence there might be some ground for the argument that the protective nature of the epithelial covering of the glomerulus is lost. This, I think, will hardly hold good, for in advanced cases of the disease there is absolutely not a single tube in which the epithelium is normal, and yet there may be only a trace of albumen in the urine. The continuous retardation to the flow of the blood expresses a greater quantity of water than usual, but it is only occasionally during exacerbations, that the obstruction is sufficient to drive out the serum albumen.

In the albuminuria of acute desquamative nephritis, the same cause appears to be at work, namely, obstructed circulation. The intense congestion one usually sees in the vessels of the medulla in this disease, points to obstructed flow through the cortex. It might be said that the quantity of urine ought here also to be increased. I should not at all be surprised if the quantity actually excreted by the glomerulus were increased, but would explain the small quantity actually reaching the bladder on the principle that it is re-absorbed, being unable to make its way down the obstructed tubules. The urine may thus be left in a concentrated condition, so that that which reaches the bladder may contain a considerably higher proportion of albumen than it did when excreted. As an actual fact, it will be familar to every one who has studied the histology of the disease that the lymphatic interspaces are all widened both in the glomerulus and in those intervening between the tubules.

In concluding my remarks, I have, Sir, finally to ask the Society for a little to consider the complications which tend to arise in the course of the various forms of Bright's disease. They are mainly inflammations of the various serous membranes and of the lungs, hæmorrhages into the retina and brain, and, in the cirrhotic form, hypertrophy of the muscular fibre of the arteries, and of the left ventricle of the heart. Many theories have been suggested to account for these; but it seems to me that all are more or less insufficient to thoroughly explain their occurrence. I shall consider them together, because I believe that they are due to a common cause.

Some time since, I showed before the Royal Society of Edinburgh a number of physical experiments bearing upon the circulation of the blood corpuscles. One of the chief conclusions derived from these experiments was that the cause of the coloured corpuscles running in a central core in the blood-vessels is that they are almost exactly of the same specific gravity as the plasma of the blood. If a body of the same specific gravity as the liquid in which it is suspended is made to travel through either a straight or a bent tube, it runs in the axial stream, while, if it is lighter or heavier than the liquid, it runs, respectively, on the upper or lower surface of the tube. The coloured blood corpuscles are almost of exactly the same specific gravity as the *living* plasma in which they are suspended, while the colourless are markedly lighter. Now, the whole essence of the blood, as a circulating fluid, is that the coloured corpuscles suspended in it never touch the wall of the smaller vessels, even of the minutest capillaries. They glide along in the axial stream without any sign of impediment. A body of the same specific gravity as the liquid in which it is suspended, further, turns round a curve in the tube with ease, while one lighter or heavier experiences the greatest difficulty in doing so, and continually tends to catch in the bends. Hence, from the great mass of the solid particulate matter of the blood being practically of like specific gravity with the living plasma, undue friction is avoided, and the blood as a whole circulates with very much the same facility as pure plasma would. When a light substance, such as oil or milk, on the other hand, is introduced into the circulation, it will not circulate because the particles catch in the curves of the vessels, more especially in parts like the lung where the vessels are particularly tortuous. Blood circulates through a capillary glass tube exactly as it does through a small vein or capillary vessel. The coloured corpuscles occupy the axial stream; the colourless the peripheral. Hence, the phenomenon must be a purely physical one. Further, the position in the stream occupied by bodies thus suspended in a liquid may be altered at will, not only by altering the specific gravity of the bodies themselves, but also by changing that of the suspending liquid. Thus, by substituting a very light liquid or a very heavy, the suspended blood corpuscles may be made at will to course along either the lower or the upper surface of the vessel at will. They come in contact with it, and by rubbing against the wall are made to roll instead of to glide. The friction thus caused retards their progress; they tend, like oil globules, to catch in the curves of the vessels, and thus to cause obstruction to the even continuous flow of the blood stream. The difficulty that may be thus caused by this apparently trivial cause in moving the blood onwards comes to be very great when estimated all over the body, and were it the case that a marked difference in the relative specific gravities of the blood plasma and the coloured corpuscles prevailed, the continuation of the even onflow of the blood with the usual propelling power would become a physical impossibility.

Now, the theory which I would propound for discussion, as explanatory of the concomitant pathological phenomena of Bright's disease, is this:—that the blood plasma has a wrong specific gravity, and that, on the above principles, the circulation is retarded. Christison, long ago, showed that the serum of the blood in acute Bright's disease is of particularly low specific gravity, sinking as low as 1020 to 1019. He further demonstrated that it is deficient in serum albumen, and that the low specific gravity is caused, in all probability by the deficiency in this constituent. These observations have been confirmed in later times by other observers, so that we may rely upon them as being correct. Any alteration in the specific gravity —either an increase or a deficiency—would give rise to serious obstructive effects, and, as a result, I hold that all the collateral diseases met with in albuminuria may be accounted for. The increased friction will necessitate a greater propelling power, hence, the hypertrophy of the left ventricle when the disease becomes chronic. There is increased strain thrown on the wall of the blood-vessels, and, accordingly, hypertrophy of the muscular coat is required, in order to keep up the tone of the vessel. Obstructions occur in small blood-vessels locally, especially in such as are tortuous (as in the retina), hence the punctiform hæmorrhages; and where this obstruction occurs over a wide area, an inflammatory effusion, that is to say, almost pure blood plasma, is thrown out.

I believe that there is a class of diseases, or concomitants of diseases, of which at present we have no conception—namely, those due to a wrong specific gravity of the circulating fluid. I am at present engaged on an inquiry into the subject, and hope before long to be able to give some reliable results.

The following microscopical specimens, prepared by Mr. H. Lyon Smith (Dr. Newman's laboratory-assistant), were shewn:—

(*a*) Fœtal kidney, shewing arteries, Malpighian bodies, and capillaries injected. The section was made through the cortex, and shewed the labyrinth (composed of Malpighian bodies and convoluted tubules) and medullary rays.

(*b*) Shewed an injected interlobular artery giving off several afferent arterioles to the Malpighian bodies.

(*c*) Shewed several Malpighian bodies with their afferent and efferent vessels, also injected. The afferent vessels were seen arising from the interlobular artery, and the efferent vessels splitting up to form the true capillaries.

(*d*) Transverse section through the cortex.

(*e*) Longitudinal section through the medulla to shew the arteriæ rectæ injected.

(*f*) Section through the medulla which shewed loops of Henle and straight tubules.

(*g*) Another longitudinal section through the medulla, which shewed arteriæ rectæ injected, large collecting tubes, ascending and descending limbs, and loops of Henle.

(*h*) Prepared by injecting carmine and gelatine mass into the ureter of a kidney at a pressure of about 60 mm. of mercury. (The kidney presented changes similar to those found in early parenchymatous nephritis.) None of the tubules contained any of the injection fluid, although there was evidence that the pelvis of the kidney had been filled by it. To the naked eye the cortex appeared coloured, although differently to what is seen when the arteries have been injected, as the colour was limited almost entirely to the capsules, extreme cortex, and labyrinths, whilst the medulla contained no injection, and the medullary rays only faint traces. Under the microscope it was seen that the injected material occupied the perivascular lymphatic spaces, but at no part could evidence be found of its presence actually within any of the blood-vessels. None of the glomeruli were injected, but surrounding their capsules were thin rings of the injection. Most of the injection had passed into the lymphatics of the capsule and of the extreme cortex; so much so that the convoluted tubules situated immediately below the capsule were entirely surrounded by fully injected lymphatic spaces. The injecting mass seemed to have found its way into the lymphatic system by rupture of the inner coats of the ureter.

CURRENT TOPICS.

New Zealand Notes.

(From a Correspondent).

The Otago University, located in Dunedin, is now fully equipped to qualify students in Medicine. Five new Chairs were lately established, and this completes the medical professorial staff of the University. Dr. E. Mackellar, a distinguished Glasgow graduate, has been appointed to the Pathology Chair. The appointments were not advertised in the home papers, and the only stranger who comes amongst us is Dr. Colquhoun, a distinguished London University graduate. He has been appointed to the Practice of Medicine Chair, and comes to the colony, I understand, for health purposes. The emoluments of the Chairs are not very handsome in the meantime, ranging as they do from £150 to £250, with class fees. However, the University has large land endowments, and as the colony becomes peopled, the value of the Chairs will increase.

Degrees in Medicine can now be conferred in Dunedin, and this is the first really New Zealand degree. Other degrees have been conferred for some time in the colony, but the papers had to be sent home for examination, thus keeping the candidates in suspense for several months. We are quite on a par with the home cities now, so far as university education is concerned. We have full curriculums, and can grant degrees in Arts, Law, and Medicine. We can also turn out full fledged divines of any denomination. Indeed, so far as education is concerned, we are abreast of the times. In medicine we have specialists for all the ordinary specialities, and indeed the science is ably and honourably represented in the city of Dunedin. No bright youth from the old country needs come here with the hope of enlightening us, or thinking to secure an excellent city practice. The man who will be successful here is the strong, able-bodied general practitioner, fit for a country practice.

Tuberculosis is likely to become epidemic amongst men and beasts in this colony. There has been a great cry amongst our squatters about rabbits becoming a pest to the country. Various means of destroying them have been tried, and the latest is infecting them with tubercle. This has been so successful that in some parts of the country the stink arising from the dead rabbits is becoming pestilential. The question shortly will be, which is the greater pestilence, rabbits or tubercle? The colonials never think of eating rabbits, so we won't die from rabbit tubercle. Mutton, however, is our staple article of food, and sheep are rapidly becoming infected with the disease. I intend becoming a vegetarian, so that I may be able to tell you if this dreaded tubercle is likely to decimate the genus homo. There is no doubt but that the disease is true tubercle, but whether it will influence the general health of the colony is a question of the future.

Talking of tubercle, I may mention that the disease is a very common one here. The young population seem not to be so robust as their fathers from the old country. Every season brings phthisical subjects into the country, and they are recommended, as a rule, a six or twelve months' residence among the mountains. Queenstown, at the head of Lake Wakatipu, a very high and mountainous district, is the favourite resort. Doubtless the lives of many persons have been prolonged by a short residence in this elevated region, and a few have been completely restored to health. Antiseptics used as inhalations are found to be very successful, as part of the treatment; but physicians generally rely upon climatic and general health principles for success in the treatment of this disease. I see that Ben Nevis has become inhabited: why not establish a sanatorium on the top of the Ben, and try the effect of high altitudes on chronic lung affections? I commend the idea to some enterprising person.

The friends of Dr. Wm. Stenhouse, a well known Glasgow graduate, will be sorry to learn that he had to submit to an amputation of the right foot lately, the result of an injury in boyhood. Dr. Stenhouse has a large practice in Dunedin and surrounding districts. He is also one of the hospital surgeons. Some three months ago he operated on a case of double ovariotomy, each tumour being about the size of a fœtus' head. They proved to be fibro-sarcomatous, and are now exhibited in the university museum. The patient made an excellent recovery, walking to her own home two months afterwards. This has been the first successful case of double ovariotomy in Dunedin. I may mention that Dr. Stenhouse has quite recovered, and has resumed practice again.

List of Candidates who were successful for appointments as Surgeon in Her Majesty's British Medical Service at the Competitive Examination in London, on 4th February, 1884.—1. J. R. Barefoot, 2,440; 2. R. H. Clement, 2,420; 3. G. D.

Hunter, 2,335; 4. W. C. Beevor, 2,300; 5. L. E. Anderson, 2,275; 6. G. B. Russell, 2,250; 7. A. E. C. Spence, 2,220; 8. J. R. Mallins, 2,200; 9. R. J. M'Cormack, 2,180; 10. H. N. Thomson, 2,169; 11. J. I. P. Doyle, 2,145; 12. N. Manders, 2,120; 13. L. R. Colledge, 2,100; 14. S. F. Freyer, 2,075; 15. C. Birt, 2,070; 16. R. S. F. Henderson, 2,020; 17. H. Mitchell, 2,000; 18. S. Butterworth, 1,990; 19. C. J. Holmes, 1,930; 20. J. R. Lane, 1,920.

REVIEWS.

Lehrbuch der Krankheiten des Nervensystems. Von Dr. Adolph Seeligmüller. Abtheilung I. Krankheiten der peripheren Nerven und des Sympathicus. Braunschweig: Friedrich Wreden. 1882.

Handbook of the Diseases of the Nervous System. By Dr. Adolph Seeligm¨ller. Part I. Diseases of the Peripheral Nerves and of the Sympathetic. Brunswick: Friedrich Wreden. 1882.

This volume, the fifth of Wreden's series of Medical Text-books, is divided into two portions—the first and smaller portion dealing generally with nervous diseases; the second, or special portion, treating of the various diseases of the individual nerves in detail. In the former we find a brief summary of the functions of the nervous system and especially of the nerves, a short account of the general ætiology and pathogenesis of nervous diseases, and a rather longer account of their general treatment. That on ætiology might with advantage have been enlarged; it is mainly occupied with the enumeration of the causes and conditions which influence the occurrence of nervous disease—among which we specially note heredity, the business hurry of the present day, the abuse of tea, tobacco, and stimulants, and the habit of daily railway travelling. On these subjects we could have wished fuller information, but they are each dismissed in a few lines.

A considerable amount of space is devoted to the general therapeutics of nervous diseases, including general treatment and remedial treatment. Here we learn that Seeligmüller looks upon syphilis as one of the most important causes of such diseases, and as often giving rise to quite anomalous symptoms; hence he formulates the conclusion that "what we cannot understand, should always be looked upon as due to syphilis." An aphorism of more general acceptance is, that "the treatment of nervous disease must be as pertinacious as the disease." In noting the therapeutic value of rest (in a very wide sense), he makes a graceful reference to Hilton's work on *Rest and Pain*, but at the same time gives a very necessary warning as to the possibility of ordering rest when exercise is what is required, with special reference to cases of hysteria.

The section on thermo-therapeutics discusses the relative values of hot and cold applications, describes with some detail the method of applying various general and local baths, and lays great stress on our power of regulating the blood supply in the brain and cord by means of cold and hot applications. Vapour and Turkish baths are dismissed in a few words, as being unduly stimulating in most cases of nervous disease.

Brief sections follow on electricity, including a description of the various appliances and of their mode of application and value in various affections, and on massage, both of which are looked upon as among the best remedies at the command of the physician. The various drugs employed against nervous diseases are also mentioned in this general part, but we find nothing new.

The special portion of the work is carried out in great detail, the signs of sensory and motor disorder of each individual nerve being given with considerable minuteness, under the various headings of—(*a*) Anatomical diseases of the peripheral nerves; (*b*) functional diseases of the peripheral nerves, including diseases of the nerves of special sense, diseases of the sensory nerves, diseases of the motor nerves; cramp in its various forms; and diseases of the sympathetic.

In the section on "cramps" we find that contractures are divided into three orders:—

I. Myopathic: *i. e.*, rheumatic, inflammatory, syphilitic, and toxic.

II. Contracture from defective position of the bones: *e. g.*, from dislocation.

III. Neuropathic—(*a*) spastic, and (*b*) paralytic: or (1) cerebral, (2) spinal, (3) peripheral, and (4) hysteric. Spastic contractures, he believes, may be produced either directly by irritation of the motor nerves at their origin or in their course, or reflexly from action on the sensory nerves. There is nothing new as to the treatment of these.

In the same section, we find mention of Blepharospasm, a great number of varieties of which are more or less minutely described, the prognosis being given as rather unfavourable, though depending on the cause. We were disappointed with the section on treatment, which seemed to us to be rather incomplete, even as regards the remedies more commonly in use. The same may be said of the section on the treatment of angina-pectoris, where no mention is made of nitro-glycerine, and very little is said as to the physiological effects of the remedies recommended. As to the pathology of angina he is more detailed, and his conclusion is, that the nervous apparatus of the heart is the seat of mischief, different nerves being affected in different cases—in some the automatic ganglia in the substance of the heart, in others the vagus, and in others the sympathetic—the extension of the pain to the shoulders being due to the numerous communications between the branches of the vagus and the sympathetic with the brachial plexus. He looks upon angina as in many cases induced by excessive smoking.

This work can be safely recommended as giving a very fair and full summary of our knowledge of diseases of the peripheral nerves, without, however, much fresh matter.

Elements of Human Physiology. By Henry Power, M.B., Lond., F.R.C.S. London: Cassell & Company, Limited. 1884.

This is one of the red hand-books which are being issued at present by the above Company. As stated in the preface, the following departments are omitted:—Histology, Clinical Chemistry, Practical Physiology, Comparative Physiology and Anatomy. Information on these subjects is supplied in other volumes of the series. It is asserted on many hands, that the medical student is over-educated, and most people agree in thinking that, as a rule, the time which the future physician can devote during his curriculum to experiment is too limited to be of almost any value. Many fear that even the microscope may be too seductive; but no one doubts that the more time devoted to the subject of this little book the better. Physiology in its broad outlines must ever remain the one "sound basis for the future practice." The essentials of the subject are all here.

Mr. Power is fortunate and unfortunate. He has been called upon to write a book to contain the physiological facts which a physician *must* know as distinguished from that which he *may* know. The latter come under the headings of the other volumes on physiological subjects in this series. Histology is more properly a branch of anatomy. The student may well feel staggered to find that he is expected to absorb the contents of the whole five; but will probably regard this as a counsel of perfection. This limitation in scope is at once the advantage and disadvantage of the volume before us. It should prove most valuable as a guide for study; but those who have unfortunately to face an examination on the whole field will fear that the ground covered is too narrow. Still, this book fully and admirably goes over all that ought to be familiar to the medical student so far as physiology can be rightly defined. The more specialised knowledge may well be left to those who have sufficient leisure.

This seems to us to be the best compendium of physiology properly so called in our language. It is, of course, a compilation. Recognised facts are concisely described. Long discussions of disputable points and doubtful theories are wisely avoided. Uncertain matters are briefly mentioned, and the opposing views stated, which gives a correct representation of the boundary of our knowledge. The balance of the different parts is most satisfactory, and shows the due proportions, which indicate an experienced judgment. In such books the style is apt to be jerky, and perhaps this is not altogether avoidable; but here the sentences, if necessarily compressed, are abundantly clear, and the facts are presented in an interesting way. Varied types, numerical divisions, tables, and short paragraphs, such striking improvements in modern manuals from the student's point of view, if not so in the opinion of the more æsthetic publishers, are freely used to aid the attention and memory. It is a book to be recommended in many ways; but the real test for such books can only be applied by those who use them in teaching or in study. It should stand this test well, and prove acceptable to those so engaged.

The scope of the work will be gathered from the following outline of its contents. A synopsis of the elements and organic compounds forming the body is the starting point, after which the blood and its circulation receive full consideration. This is followed by respiration. Food, with its digestion and absorption, is the next subject, and the space occupied is in keeping with its manifest importance. This section is a most excellent one. The glycogenic function of the liver has a short chapter to itself, followed by one on the skin. Animal heat is then taken up, and our knowledge of this clearly stated. The nervous system next occupies attention, and this, which, like the system itself, branches out into almost all other departments of physiology, is followed out in all directions. The different senses follow naturally. As might be expected, the author dwells on the eye and sight with the fond freedom of one to whom the matter is very familiar, and this treatment deserves special praise. Generation and development come last. An appendix suffices for remarks on some substances not described in the text.

Dr. George Johnson would be grieved to find that picric acid is not given as a test for albumen. The following points happened to catch our eye:—Figure 25 is hardly correct; the images K and L would be farther forward than the points F and F′. At the latter points the images would be lines not ovals. Hæmatoidin crystals are said to have the same form as those of Bilirubin, but the latter are not described. Saliva is said to contain "salivary corpuscles, which perform amœboid movements." This can scarcely be made to agree with Klein's accurate description of the bodies in question. When will the ghost called "salivary corpuscle" be laid?

The Dissector's Manual. By W. Bruce-Clarke, M.A., M.B., F.R.C.S., and Charles Barrett Lockwood, F.R.C.S. London: Cassell & Company, Limited. 1883.

Teachers of anatomy have long felt the want of a reliable and well written Manual of Dissections: for the classic work of Ellis is too cumbrous, too tautological, and too much behind the science of the present day, to be placed with any confidence in the hands of a student. The Manuals of Heath and Holden, on the other hand, are very unsatisfactory from the scantiness of detail, the baldness of statement, and the general incompleteness which characterise them; they lead the student only into the "outer court" of the science and then leave him, just as he requires the most assistance and the most information. Cunningham's section on the abdomen was a model of what a dissector's manual should be, and we regret that it remains a fragment of what we fondly hoped would have been a complete work. We are sorry that the work before us does not in any measure fill the void. Its title held out a promise which the contents do not redeem, and we regret that Messrs. Clarke and Lockwood have expended so much time and energy in doing a work which was done equally well by Cleland years since. We are convinced that it is a mistake to separate the directions for dissection from the description of the structures displayed, for the student is either obliged to carry with him two books, or he fails to identify, and consequently destroys tissues which such a manual as the one before us does not enable him to recognise. Our authors, indeed, are not ignorant of this, as is clearly shown by such recommendations as the following:—"Before dissecting the ulnar and radial arteries their branches should be learnt, and afterwards found. Unless this is done in the case of *every artery*, its branches are exceedingly likely to be severed from the main trunk" (p. 35). "Before endeavouring to make out the various structures, the student is strongly advised to make himself familiar with their relations. The diagrams contained in his anatomical text-books should be consulted. . . . ." (p. 234). But while thus taking exception to the design of the work, we are not altogether satisfied with the way in which the authors have carried out their plan. Surely, if the student is so ignorant as to require a glossary to inform him of the meaning of the words "clean," "dissect," "ligament," "demonstrate," and "anatomy," he would need a dictionary of greater length than the thirty-three words herein gathered together for elucidation. Why should it be necessary to explain what we mean by "flex" and "extend" when it is assumed to be unnecessary to give a translation of "abduct" and "adduct," or why should the reader be assumed to have a knowledge of an "inosculation" when he is ignorant of an "anastomosis?" The glossary should have been left out, or else so framed as to be of real value. We cannot see the use of the two illustrations of dissecting forceps and blow-pipe; the student generally purchases these articles long before he thinks of a dissector's manual, and some months before commencing practical work in the dissecting room. The "mode of preparation" might also have been spared, although we would in passing commend highly the method of preservation and arterial injection herein described; in our hands preparations very similar have given most excellent results. Such details are, however, of no use to the student, and occupy space which might have been used to more advantage in other ways.

We thoroughly endorse the advice given in the introductory chapter, and would commend it to the attention of all students, who are commonly too ready to forget that it is about as wise to build a house without a secure foundation as to commence to dissect before a thorough knowledge of the bones has been obtained; and who are so willing to credit the writer of their text-book with a profound knowledge of his subject that they rarely take sufficient trouble to verify the facts therein set forth. A judicious scepticism should always be exercised by students of science in all its departments, and certainly not less in anatomy than in other subjects.

The directions for dissection, which form the bulk of the book, are for the most part full, accurate, and judicious, and there are few noteworthy errors. We miss in the dissection of the thorax the admirable method of displaying the pleural sacs and the anterior mediastinum devised by Turner, and described by Cunningham; and in the dissection of the orbit we note that the authors adhere to the old clumsy, difficult, and destructive mode of inflating the eyeball through the optic nerve, instead of adopting the simple and effective plan of puncturing the cornea as suggested by Hensman. It is to be regretted that the names "occipito-atloid," "occipito-axoid," and "condyloid" have been retained, instead of being substituted by the more elegant and descriptive "occipito-atlantal," "occipito-axial," and "condylar"; and a similar comment may be made regarding the attolens *aurem*, which should be *auriculam*, that is to say, muscle of the *auricle* or external ear. But while new titles are thus ignored, we have in compensation the restoration of an old name for the space above the upper border of the superior constrictor muscle of the pharynx, which is called the *sinus of Morgagni;* although we have ourselves used this name for years, we have been sorry to find it generally omitted in recent editions of standard works, and most notably in Quain's *Anatomy*.

The descriptions of the dissection of the eye, ear, nose, and larynx, of the brain, spinal cord, and Meckel's ganglion call for comment as being specially well written, and as giving directions and particulars rarely found in text books.

It is claimed for the illustrations that all excepting two are original, but in consideration of the general want of artistic merit, and often of anatomical accuracy, the authors might with advantage have drawn upon the store of admirable diagrams found in other works.

Medical Diagnosis: A Manual of Clinical Methods. By J. Graham Brown, M.D. Second Edition. Edinburgh: Bell & Bradfute. 1883.

In the number of this *Journal* for September, 1883, the first edition of this *Manual* was noticed. Since that time a second edition has appeared, differing from the former mainly in additions to the sections on the use of the laryngoscope, the ophthalmoscope, and electric instruments in diagnosis, in the introduction of some woodcuts, and in the omission of the chapter on the reproductive system. The additions made are in the way of improvement; but they are not enough. Like other writers on medical subjects, Dr. Brown practically ignores the ear and its diseases. While the ophthalmoscope and the appearances of the retina are described, no mention is made of the aural speculum and the membrana tympani. Surely deafness is sufficiently common and sufficiently important to demand that some attention should be paid by the clinical teacher to the membrana tympani, &c., as found in disease. Another omission that must be noted is rather striking at the present time: there is no mention made of picric acid as a test for albumen or sugar in the urine. The woodcuts added will be of use to the student, but they cannot be described as artistic productions.

Clinical Notes on Cancer. By HERBERT L. SNOW, M.D., (Lond.) Surgeon to the Cancer Hospital, Brompton. London: J. & A. Churchill. 1883.

THIS book opens up an interesting and ever recurring question—the value of the hereditary factor in the production of cancer. That there is such a factor is a widespread belief; it is taught in all our schools and textbooks, urged by our most eminent pathologists and scientific observers, and seems most intimately bound up with the great doctrine of evolution. But Dr. Snow's experience has led him to denounce it as a fallacy most pernicious alike to surgeon and patient, and he here attempts to explode it.

From his opportunities at the cancer hospital one would have expected that whatever position he took up he would have been able to support it with numerous facts arranged in a formidable manner. But we must confess that his book has sadly disappointed us, and has added absolutely nothing to our knowledge of the etiology or transmission of cancer.

The facts upon which Dr. Snow bases his opposition to the "heredity-fallacy," as he is pleased to call it, are—

1st. He finds that out of 358 cases of breast cancer only 54 gave some account of cancer in at least one member of their families, and 7 were doubtful.

2nd. Of 268 uterine cases 23 gave a family history, and 11 were doubtful.

3rd. Of 108 cases of epithelioma 4 gave a family history, and 2 were doubtful.

4th. Of 52 patients who presented themselves at the Cancer Hospital with complaints (all tumours being excluded) in no way allied to cancer, 13 affirmed that at least one relative had died from cancer. He adds, however, as regards the last class, that "in people coming to such an institution large allowance must be made for the preconceived idea;" while, as regards the others, he thinks that from the imperfect knowledge of most people as to the maladies of relatives dead many years before, he is entitled to reduce the above percentage of patients with a cancerous family history very considerably. From these facts he concludes, without any more ado, that "the theory of hereditary tendency proves baseless" (p. 24) and that the sooner such a baneful hypothesis is given up the better. It ought no longer to be allowed to excite the fears of patients or to obscure the "scientific vision" of the surgeon and restrain his hand.

This is a conclusion, however, which we can by no means accept. The facts adduced leave the argument for heredity entirely untouched. Dr. Snow acknowledges that 15 per cent, or 1 in every $6\frac{2}{3}$ of his breast cases gave a family history of cancer. This observation agrees pretty closely with the statements of Paget,* Baker,* and Butlin,† that a family history of cancer may be obtained in 1 out of every 5 or 6 hospital cases. Sir James Paget's tables,‡ including both hospital and private cases, showed that 24 per cent, or nearly 1 in 4, gave a family history. But in private practice alone, owing to the more accurate information on family matters possessed by the wealthier classes, the proportion was stated by Paget to be still greater—1 in every 3 cases. On this point he makes the following striking remark §:—"Every year's experience in practice among persons whose family histories are unknown makes me more sure that inheritance is the great power in the production of all diseases that are not of distinctly external origin, and among these, of cancer."

But the point of Dr. Snow's objection is, that the negative evidence of 85 per cent of his patients entirely overwhelms the positive evidence of 15 per cent. Now it seems to us that it really does not matter whether the negative is supported by 85 per cent or 75 per cent, as in Paget's cases, if that negative simply implies ignorance, as it does here. If we consider carefully the general ignorance on such family matters which Dr.

* *Path. Trans.* Vol. XXV, p. 317.
† Holmes' *System of Surgery.* Vol. 1, p. 245.
‡ *Medico-Chir. Trans.* Vol. XLV, p. 397.
§ *Lectures on Surg. Path.*, p. 792. Note.

Snow has adverted to, and which must operate with much greater force against the negative than against the positive evidence, from the well known facts that inherited diseases and other characteristics may overleap one or more generations,* or may attack internal organs, or may only appear after a certain time of life, and so patients may die from other causes before the evolution of the inherited disease, it will be seen how true are Paget's words—"When any one says that no instance of this or that disease has ever occurred in his family, the statement is hardly worth recording, even though it be made with much more than usual consideration."† Then what are we to make of such cases as that of the family mentioned by Broca‡ in five generations of which sixteen persons out of twenty-six, who lived beyond the age of thirty, died of cancer? Warren§ gives a similar instance, where the grandfather died of a cancer of the lip, a son and two daughters died of cancer of the breast, a granddaughter of cancer of breast and uterus, and a grandson of cancer of breast. Paget|| gives the following:—A lady died from cancer of stomach, one daughter from cancer of stomach, another from cancer of breast; two grandchildren died of cancer of breast, two of cancer of uterus, one of cancer of bladder, one of cancer of axillary glands, one of cancer of stomach, and one of cancer of rectum. What are we to make of such cases? Do they not prove the fact of inheritance? Dr. Snow says such cases are "very much the exception to our daily experience, and cannot be taken to prove a law when in so particularly marked a minority." Perhaps not, if the evidence of the majority had the slightest positive value.

Dr. Snow, moreover, thinks if we were well rid of this pernicious heredity-fallacy it would save the surgeon from "errors of diagnosis whose consequences to the unfortunate patient (to say nothing of his own repute) cannot be over-estimated." We would like to ask if this statement is based on Dr. Snow's own experience, because it seems to us to be more of a bugbear than anything else. We can suppose such errors of diagnosis leading to one of two consequences, either a simple tumour might be mistaken for a malignant one and

* Baker in *St. Barth. Hosp. Rep.*, Vol. II, p. 136 says:—Of 103 cases of transmission, 16 were instances of inheritance from a grandparent or a great grandparent, or both.

† *Clinical Lectures*, p. 366.

‡ *Traité des Tumeurs*, p. 151.

§ *Surg. Observations on Tumours*, p. 281.

|| *Path. Trans.*, Vol. XXV, p. 319.

removed, not a very terrible misfortune for the patient; or a malignant might be mistaken for a simple tumour, and so operation deferred. This would be a worse mistake than the former, for the surgeon at least. But would it be any worse for the patient? Have we not all failed most completely hitherto in our efforts to eradicate cancer? Have our ablest surgeons, with the most refined diagnosis, and the most perfect technical skill, anything but disappointment to confess to, or to look forward to, in such attempts? Can Dr. Snow himself give us the particulars of any cases of true cancer which have been cured by operation, however early and bold? If he can, and will do so in the next edition of his book, they will form the strongest possible arguments in favour of the position he has taken up.

But having disposed of the heredity-fallacy, Dr. Snow tries to establish a theory of mental worry as being the true cause of cancer. In support of this view, he first of all brings forward certain theoretical considerations based in some unexplained manner on the argument from design and Darwin's theory of evolution. Thus, he says, "Cancer (of course excluding epithelioma) is, teleologically speaking, a disease of the nervous system;" adding truly enough that this "is, at first sight, a somewhat startling hypothesis." Nor is it less startling when he proceeds to show that it is in harmony with "the great conception which we associate with the name of Charles Darwin." But we need not follow Dr. Snow in his philosophical exercitations. Every one acknowledges the supreme importance over nutrition of the nervous system, and is quite prepared to accept the statement, if properly supported by actual observations, that mental distress is an important factor in the causation of cancer, as well as of other diseases of nutrition. But we are not prepared to go the length of Dr. Snow, and say that it is the only such factor, or even the only form of this neurotic factor. May not, for example, a morbid irritation of the uterus be reflected injuriously on the breast, or *vice versâ?* Or, again, may not the general depression of the entire generative system after middle life of itself be sufficient to produce in these organs such a morbid alteration of nutrition as to lead to cancer?

To sum up, there may be something in the theory of the local origin of cancer, and in the malignant influence of mental emotion; the heredity theory also may be a fallacy, but if Dr. Snow's book is meant to establish the former and explode the latter, "teleologically speaking," it is a failure.

Baldness and Greyness; their Etiology, Pathology, and Treatment. By Tom Robinson, M.D., Physician to St. John's Hospital for Skin Diseases. Second Edition. London: Henry Kimpton. 1883.

This is a very well written little work, and gives a most full and interesting account of the subjects of which it treats. Without any lack of scientific accuracy the book abounds in little historical, and sometimes amusing, details, which greatly relieve the tedium sometimes felt in reading a purely technical treatise, and this, combined with a very pleasing style, imparts a great deal of pleasure to its perusal. The work consists of seven chapters on the following subjects:—Anatomy of the Hair—Physiology of the Hair—Colour and Texture of the Hair—Alopecia—Canities, Piliosis, Greyness or Hoariness of the Hair—Crinal Abnormalities—Treatment.

We have much pleasure in recommending it to our readers.

Transactions of the Academy of Medicine in Ireland. Vol. I. 1883. Edited by Wm. Thomson, M.A., F.R.C.S., General Secretary. Dublin: Fannin & Co.

Towards the close of the year 1882 the existing Medical Societies in Dublin amalgamated and constituted "The Academy of Medicine in Ireland," and the volume at present under consideration is the first annual issue of its Transactions. From the preface we are pleased to learn that the various sections have worked regularly, and that the business of the new organisation has proceeded with much smoothness.

At the beginning of the book we find a list of officers, fellows, members, and student associates; and on referring to the rules we find that students beyond the third year are elected as associates, and have the privilege of attending the ordinary meetings of the society. We think this movement a very good one, and one likely to give to the students an enduring interest in the school in which they have been trained, and we are glad to observe that a considerable number have availed themselves of this privilege, which they may obtain for the very small annual subscription of 5s.

The academy is divided into a medical, a surgical, an obstetrical, and a pathological section; and there are subsections on public health, anatomy, and physiology. Under each of these sections we find recorded a large number of interesting and valuable papers.

The volume itself is a large and a handsome one. It is well finished in every respect, and contains a number of excellent illustrations. The new movement is a strong indication of the energy and enterprise of the profession in Dublin, and we heartily wish it success.

Opium Smoking and Opium Eating; their Treatment and Cure. By George Shearer, M.D., Senior Assistant Physician to the Hospital for Consumption, Liverpool.

That the Opium question, especially as it affects China, is attracting some attention in certain quarters seems to be borne out by the fact that this is the third work on the subject that has been brought under our notice within the last two years. Some time ago, under the title of *The Other Side of the Opium Question,* we published a short notice of a work by Surgeon-General Moore, and the object of this book may be compared with that of the present one, if we give a sentence or two from each. Dr. Moore says—"I am actuated by the firm impression that the British public are being misled by probably well meaning but certainly mistaken persons, who erroneously regard the use of opium as the crying evil of the times. Should such persons succeed in their endeavours, the result could only be increased taxation on the people of India, which neither they, nor, in these days of depreciated rupee, the Europeans employed in the country, could endure." Against these statements the reader may place the following from the closing sentences of Dr. Shearer:—"*What will raise China?* The paralysing influence of idolatry must be shaken off; the sleep of the ages must be broken. Thus only, we believe, will she be enabled to take her true position and rightful place amongst the great nations of the earth. A nation of immemorial antiquity and considerable native capacity, but still only a nation of undeveloped men, with the feeble virtue of mere adolescents, she has on this account only the stronger claim upon the sympathy and compassionate succour of such a people as the English, more powerful, more advanced than they in point of privilege. . . . But should England decide still to continue this nefarious traffic, she shall duly reap as she has sown, since there is a God that judgeth in the earth. Too long in this matter of the opium traffic with China has she proved herself unworthy of all her divine teaching and past traditions." It would seem, therefore, that to the opium, as to all other questions, there are two sides; and, although it is not our intention to endeavour to bias our readers in favour of one side or other, we would advise those who are interested to peruse both works, each being tolerably characteristic of the party to which it belongs.

The present work is a small volume of some 157 pages, and goes very fully into the whole subject. Quotations from various authors are abundantly given, and De Quincey's statements and opinions are dealt with in great detail. In conclusion, the author gives many valuable hints as to the treatment of the opium habit. In many places the book is suitable for the general as well as the professional reader, and, although we do not very much admire the style, or like the way in which the work is split up into fragments, we must say that a very large amount of valuable information is contained in its pages.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

An Instrument for Temporarily Closing either Ureter.—Silbermann (*Berlin Klin. Wochenschr.*, 1883, No. 34) figures and describes a new instrument by which either ureter can be closed long enough to allow of unmixed urine from either kidney to be collected for examination. (See Dr. Newman, in *Glasg. Med. Jour.*, Aug., 1883.)

The instrument consists of a metallic double catheter, with quick curve, and a window $3\frac{1}{2}$ cm. × 5 mm. near its point. The window can be closed by a shutter moved by a stilet. Within the catheter there is contained a firm elastic catheter, which terminates in a small expansible gutta percha balloon or tampon. When collapsed this tampon is contained within the point of the catheter.

The instrument is passed into the bladder till its beak impinges upon the posterior wall in the middle line, guided by a finger in the vagina or rectum (in men). The window is then opened, and the tampon made to expand by pouring mercury into it through the elastic catheter. By the introduction of 20 ccm. of mercury, which weigh about 270 grms., the tampon can be filled up to the size of a goose's egg, and guided by the finger to either side of the middle line, will by its weight effectually prevent any outflow of urine from the ureter upon that side. The bladder can be emptied and washed out through the other channel of the metallic cannula, and urine from the free ureter allowed to collect for from fifteen to twenty minutes, in which time quite enough for satisfactory examination can be obtained.

Silbermann had used it twenty-two times in women, and five times in men, and did not find its use attended with discomfort to the patient or followed by any evil effects.—*Centralbl. f. Chir.* 1883. No. 43.—D. M'P.

Remarkable Obstetric Case — Abortion at Two Months and Quadruplets at Full Term. — By Drs. Edwards and M'Taggart, London, Ontario. On the 21st of July, 1883, we were called to see Mrs. S. of this city. Patient of small stature; English by birth; æt. 38; average weight, 100 lbs.; height, 5 feet 1 inch. She is the mother of four living children, two boys and two girls—aged 12, 10, 8, and 7 years. There was nothing unusual at any of her previous confinements—never had an abortion before. On abdominal examination, we found the abdomen extremely enlarged and pendulous. We advised support from the shoulders. She told us that she was but five months *enceinte*; but from her history and condition we assured her that she was seven months pregnant. Patient always enjoyed good health; menses always regular. She last menstruated on 4th Dec., 1882. About seven weeks from this time she commenced to flow, which lasted for some three weeks; this was accompanied with pain. With a pain somewhat resembling a labour pain something was expelled, which she described "as a lump of flesh with blood-vessels in it." To this "lump" was attached a short "string." At this she became alarmed and consulted a medical man, who assured her that she had had a miscarriage. He prescribed some medicine, which he said would check the flow and remove anything that might remain. From her account the flow increased for a few days, but finally stopped.

From this time until Friday, 14th Sept., 1883, she has been, comparatively speaking, quite well, although distressed by the immense size and weight of the abdomen. On the above mentioned date she was delivered of four living children—two boys and two girls, the time elapsing between the birth of the first and birth of the last child being one hour and forty-five minutes. The weight of the male children exceeded that of the females by a few ounces, the weight of the males being 4 lbs. 9¼ oz. and 4 lbs. 3 oz., and that of the females

4 lbs. 6 oz. and 3 lbs. 13¾ oz. Labour terminated favourably, there being no hæmorrhage to speak of. There was but one placenta, and each cord was inserted at different places on its surface.

The quartette are now six days old, all healthy and able to nurse, and all bid fair to live. The mother is doing exceedingly well, having suffered no more exhaustion than if she had had but one child. We might here say that the father, Mr. C. S., is English by birth, æt. 41, height 5 feet 5 inches, and average weight 169 lbs., is a strong, healthy, and robust man.—*Canada Lancet.* October 1883.—J. L. S.

A New Menstruum for Chrysophanic Acid.—Professor Auspitz, of Vienna, has recommended liquor gutta percha as a convenient and satisfactory vehicle for the external application of chrysophanic acid in cases of psoriasis. In a paper read before the Philadelph. Co. Medical Society, Dr. Van Harlingen stated that he had used a 10 per cent emulsion, painted on once daily. He had found it an active preparation, and it had the great advantage that once the solution had dried on the skin, which it did very quickly, there was no risk of the surrounding skin being stained, or the bedclothes soiled.—*Boston Med. and Surg. Journ.* 8th Nov., 1883.—D. M'P.

Vesico-vaginal Fistula. — R. Kaltenbach gives a case of severe vesico-vaginal fistula, in which he succeeded in rendering the patient comfortable by making a recto-vaginal fistula and shutting up the external orifice of the vagina. The following are the chief details:—J., æt. 25. Four previous operations for the cure of the fistula had nearly destroyed the urethra, and the author used up all that remained in the performance of other two unsuccessful ones. He then tried to repair the defect by a flap operation, but was foiled by the necessity of plugging the vagina for severe secondary hæmorrhage. As a last resource he proposed Rose's operation to his patient, whom he justly styles "die Vielgeprüfte Pat." This offer was accepted. On 29th August a recto-vaginal fistula was made, the edges of the rectal and vaginal mucous membranes being stitched together. Then episiokleisis, or closing of the vagina, was attempted, and after the fourth operation on 26th October, was successful. The ability to retain urine was thenceforward complete. In November the state of the patient was satisfactory. Urine was retained 3-4 hours, and was passed from the rectum without much admixture of fæcal matter. A firm stool was obtained regularly once a day. Menstruation had taken place painlessly per rectum, and the patient had gone back to her ordinary duties. *Centralblatt für Gyn.*—W. L. R.

Surgical Expedients in Emergencies.—*The Boston Med. and Surg. Journ.*, Aug. 1883, reproduces from the *Polyclinic* the following, among other extracts, from an ingenious and instructive paper, by Dr. R. J. Levis, describing various rough and ready expedients resorted to in urgent cases when ordinary appliances were not at hand.

In a case of retention of urine, when he had not a catheter at hand, recourse was had "to a piece of iron bell wire, bent double on itself, and the blunt doubled end passed rapidly into the bladder. The distension of the urethra by the doubled wire allowed the urine to pass freely between the wires."

"A female catheter may be extemporised from a short piece of rye straw, the end of which is to be closely wrapped for a short distance with thread; or, the end of the straw may have its sharpness removed by dipping into melted sealing wax. The stem of the ordinary clay tobacco pipe is also efficient for the purpose."

When bleeding was required, but no lancet at hand, "having put on the usual constricting bandage to distend the veins, I first transfixed the most prominent vein with a fine needle. Thus held securely, it was very easy," even with a blunt pointed and dull pocket knife, "to cut a valvular incision in the vein."

For the arrest of nasal hæmorrhage "a piece of sponge with a cord attached was pushed back with a probe or dressing forceps, through the nostril quite back to the faucial orifice, and pieces of sponge, threaded on, were slid back, one at a time, until the nares is tightly filled."

"A method of making unirritating and painless pressure within the nares, in cases of obstinate epistaxis, is by a piece of intestine of a chicken or other small animal, about twelve inches long, partially filled with either air or water. One end of the intestine is, while empty and collapsed, pushed backward through the nares; when thus lodged the air or water in the other end is forced, by compression with the hand from the pendulous portion, into the part lodged in the nares. Strong, equable compression can thus be made, rendering hæmorrhage impossible."

"As a very efficient substitute for Esmarch's elastic bandage, I suggested, some years ago, in the *Philadelphia Med. Times,* the use of a bandage made from ordinary flannel, cut cross, so as to increase its elasticity. Such an elastic bandage, from a material almost everywhere at hand, is, I know from experience, perfectly effective."

"The hæmostatic action of hot water does not seem to be sufficiently known and appreciated among practitioners. It is so effective, and can be so readily applied, that it may well displace from practice all other hæmostatics. Water, at a temperature not beyond tolerance of the immersion of the hand in it, which is a temperature of from 115° F. to 120° F., is ordinarily all that is necessary; but in some cases not amenable to treatment by ligature, a temperature above 160° F., the coagulating point of albumen, may be necessary."

"For a readily made fixed dressing a plan I have resorted to is with ordinary sand paper as the material. The sand paper is dipped into hot water, to soften the paper and the glue, and it is then applied and retained with a bandage. The glue of the sand paper soon gives rigidity; body and firmness are produced by the sand and the paper."—D. M'P.

Paraldehyde: a new Hypnotic.—The physiological action of this drug was first studied by Cervello. After experiments made upon animals this author tried its action on man, by taking four grammes in water within an hour. He states that its effect was a great tendency to sleep without alteration of the pulse or respiration, and without headache. Ultimately, it was administered to other persons, sick and healthy, and it would appear that although one gramme may sometimes produce the desired effect on a female, that three to four grammes may fail in a healthy vigorous man. It is further asserted that ten grammes in divided doses may be given to an adult without accident. According to the experiments made upon animals the quantity of paraldehyde required to obtain the hypnotic effect may be stated to be three times that of chloral. The drug is best administered in water, to which a little sugar has been added, in the proportion of 3 per cent of paraldehyde.

Since the experiments of Cervello others have tried this agent, and in the *Annales de la Société Médico-Chirurgicale de Liége* a *résumé* will be found of what has been done by Morselli, Paretti, Berger, and Masius. The first named gentleman states that he has administered paraldehyde in 350 cases. According to him 3 grammes brought on sleep of from 4 to 7 hours' duration, and that beginning within 20 to 30 minutes after administration.

Berger has observed nausea, vomiting, and vertigo—but so rarely that he attaches no importance to their occurrence. He further adds that paraldehydé is most useful where chloral has failed.

Paretti holds that it possesses the following advantages over chloral:—(1) Even in large doses it does not dangerously affect the heart's action; (2) there are no convulsive effects; (3) it brings on sleep rapidly, and without any early stage of excitement; (4) There are no disagreeable sensations on awakening. On the other hand, its disadvantages are:—(1) Tolerance is speedily established; (2) it imparts a disagreeable odour to the breath; (3) its price.—*Le Progrès Médical.* 5th January, 1884.—J. M.

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Die Krankhaften Veränderungen der Haut und ihren Anhangsgebilde, mit ihren Beziehungen zu den Krankheiten des Gesammtorganismus, dargestellt von Dr. H. v. Hebra. mit 35 Abbildungen in Holzschnitt. Braunschweig: Verlag von Friedrich Wreden. 1884.

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MEETINGS OF SOCIETIES.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

DISCUSSION ON ALBUMINURIA, ITS PATHOLOGY AND CLINICAL SIGNIFICANCE.

Session 1883-84.

Meeting VII.—*19th February, 1884.*

The President, Prof. M'Call Anderson, *in the Chair.*

Bright's Kidney in Relation to Prognosis—Anatomical Types and the Question of their Unity or Variety —"Physiological Albuminuria."

By PROFESSOR GAIRDNER.

The discussion, so far as it has gone hitherto, has been so satisfactory to me individually that I certainly feel that it is not of my own will that I intervene at all. I have repeatedly requested the Secretary to be excused, but he thought that it was absolutely necessary that I should say a few words, and therefore I feel bound to do so. I may take the liberty of saying, with regard to Dr. Newman —what I should not venture to say—indeed, I should think it presumptuous for me to say—with regard to the two gentlemen who followed him—men of so established and high a reputation—but, with regard to Dr. Newman, I may venture to say that his opening address was, it appears to me, exceedingly interesting and ingenious in its argument, and altogether creditable to himself and to the Society; though, whether it is to be accepted fully or not it is beyond me to say. I shall not trench upon those profound discussions raised by Dr. Newman's arguments almost at all. From the earliest period—we may almost say from the time of Dr. Bright—the subject of this discussion has been what may be called a kind of pathological *crux*. It has been a subject full of interest and mystery, and one where we have all been working along with various degrees of difficulty, and with a sense that we were grasping only broken lights and fragments of the truth. I believe that when the full and true pathological significance of albuminuria is understood there will be quite a new aspect of the whole pathology of what is commonly called Bright's disease. Previous to the time of Bright, albuminuria was considered a mere secretion or transudation of blood serum. Was this an error? The older pathologists, Blackall, Wells, and the others who had partially anticipated Bright's discoveries, had thoroughly recognised almost every separate truth that Bright taught, except one. They recognised the connection of albuminuria with dropsy, and the fact that it was an important and significant symptom, and that it divided the dropsies into two distinct divisions—viz., those that were connected with diseases of the organs, *i. e.*, the liver, heart, and so on, and those that were inflammatory, and not so connected. It is plain that they looked upon albuminuria as a sort of functional disorder, a sort of secretion or transudation of the blood serum only. Are we quite sure that they were wrong, and if so, how were they wrong? Modern researches, from Dr. Bright onwards, have pronounced them wrong in the sense of omission. They had omitted to observe what he taught and established by a weight of evidence that, in spite of opposition, has maintained Bright's reputation and the substance of his teaching unimpaired to the present day—that albuminuria, while it is, in fact, a secretion or transudation of blood serum, is not a mere secretion (if by that word we are to understand a functional disorder); but that it is also in most instances the clinical representative, the sign or token of certain organic changes in the kidney itself, which he partially described as seen by the naked eye, and which have ever since been the subject of most earnest discussion: in other words, that which to the pathologists before Bright appeared to be a mere leakage, meaning nothing more—meaning, perhaps, such a condition of the kidney as was implied in a mere catarrh, assumed in Bright's view an additional kind of importance as being the accompaniment of organic changes in the kidney, which again he showed to be associated with, or perhaps followed by, numerous and important organic changes in almost all the other organs of the body—inflammations, dropsies, hæmorrhages, &c. That, I believe, is a correct representation of the immense revolution which Richard Bright has wrought in pathology.

Now, there seems to be no doubt—and Rayer remarks upon that in his elaborate historical review of the whole subject—that Bright did, to a certain extent, omit to connect with his main argument what we now call the inflammatory, acute, and sub-acute forms of renal dropsy, even as they were known to his predecessors;—those, for example, to which Blackall and Wells had directed special attention—the albuminuria of scarlatina and other acute diseases. He omitted to connect these by a distinct chain of links with the organic changes of a more chronic kind, to the description of which his name is chiefly attached. Whether this omission was quite designed or not does not appear. I don't think he lays down any law, or delivers any very positive opinion, on the subject. He intimates in various places that the changes he has described in the kidney may possibly be inflammatory in their beginnings. He does not say they are; he does not say they are not. He leaves it as it were an open question, and he also leaves it an open question whether the various kinds of organic alteration, especially the more permanent and the more irreparable of the alterations which he described, were connected with each other in point of sequence or not—in other words, whether we are to admit, in essence, only one, or several, kinds of what is now called—although he did not so call it—Bright's disease. Bright, in short, did not dogmatise. He took particular care not to go beyond the limits of his own experience. He taught what was absolutely certain ascertained fact, and he did not go very far into speculation beyond that. Since Bright's time, the tendency has been all towards subdivision and toward analysis—splitting up Bright's disease into various kinds of diseases, and those who are old enough to look back for 30 or 35 years must remember that the tendency was to assume one or other of these organic alterations as being the only true Bright's disease, and to disown as such all the others. I remember when it was said that fatty degeneration of the kidney was the only true Bright's disease, and all other kinds were mere accident. This was very soon shown to be an error. However, there is no doubt whatever in my mind that Bright's researches, and especially those carried out by some of his followers, have tended to error in one direction—and that error is simply a reproduction of one that has been seen in all departments of pathology—that is to say, to attach too much importance to the fatal cases and to look too little to the cases of incipient disease and those that are followed by recovery. There is no doubt that the immense importance attached to the pathology, *i. e.*, the pathological anatomy, of the kidney since the time of Bright, has tended to create an impression that what we call Bright's disease is more hopeless and more incurable than it actually is; and this is the same tendency that has been observed in regard to phthisis and disease of the heart. It has been over and over again observed in the history of medical research that the first flush of pathological enthusiasm has for one of its results to concentrate attention on the dead body, and thereby to throw into the background the early stages and the curable forms of disease. In the case of phthisis, and possibly also in the case of heart disease, pathological anatomy has been able, to some extent, to retrieve itself by more assiduous and complete study of the subject than at first—in other words, it has been able to show, even apart from clinical research, in terms of the dead body as it were, the processes of cure as well as the processes of fatal disease. But it is doubtful whether Bright's disease has taught us thoroughly in this way even now. It is quite true that we meet with kidneys where there are irregular surfaces and irregular distribution of the vessels, but it is up to this moment a very doubtful thing whether you can attribute to any of these, in the absence of clinical history, the credit of being a case of cure of Bright's disease. Therefore, the gloomy prognosis, the gloomy aspect which has been given by the relatively too assiduous attention to pathological anatomy, and by our not being always able to follow out for a sufficiently long time our clinical experience tending in the direction of cure, still clings to the pathology of this very important subject. Nevertheless, I think every practitioner who has had a large enough and long enough experience, and who has been able to keep many of his cases long enough in view, must have satisfied himself that very ugly looking cases of Bright's disease, and even cases of the forms that are most regarded as incurable, sometimes come to something like a suspension, or, at all events, to a modified cure for the time during which they are under observation.

I have taken a note of various instances of facts bearing on good prognosis or relative cure. I published one or two in the *Glasgow Medical Journal* of February, 1878, and I have several cases, some of which were also seen by Dr. Finlayson, of our old students, whom we have been able to keep in view for many years—cases which permit us to say that even ugly looking cases of Bright's disease are amenable to a real and permanent cure. Therefore, the hopeless pathology of the disease is by no means to be affirmed as the result of our experience.

The statement by Dr. Roberts of the cure of hypertrophy of the left ventricle is a very remarkable and exceptional one; but I have seen enough in the course of my experience to quite guarantee it as a probable statement, even if it did not rest upon so excellent an authority as Dr. Roberts. I have seen numerous cases where there had been a movement in the direction of cure of hypertrophy of the left ventricle. Again, with regard to the waxy kidney, I heard, upon very good authority, some years ago, of a case in the Westminster Hospital, where a patient with enlarged liver, with albuminous urine, with enlarged spleen and a long standing disease of the femur or knee joint, or some bone of the lower extremities, was abandoned by nearly all the surgeons as too hopeless a case for operation; but where an enterprising surgeon operated, and the result was that not only was there a recovery from the operation, but that the waxy degeneration of the organs entirely disappeared, and the patient was restored to health; and I think we are perfectly authorised in believing that, even as regards the waxy degeneration, we have heard too much of the forlorn aspect, and too little of the hopeful side. At the same time, no one would say anything else than that the prognosis of Bright's disease, except in the most acute and transitory forms of albuminuria, is very grave, and I don't think that any statement I have made is opposed to a general recognition of that fact.

Again, it does not appear to me quite clear that you can prognosticate the gravity of the case by merely having regard to the anatomical type of the disease. It has been generally said that tubal nephritis is the most curable. That may be true to a certain extent, because no doubt tubal nephritis is the most acute as a rule; but what shall we say of those remarkable researches of Dr. Klein, where he shows that in scarlet fever, even on the second day and from the second day onwards, before there is albuminuria, before there is any symptom whatever specially referable to the kidney, you have, not tubal nephritis, but the interstitial form of kidney disease—the so-called cirrhotic, atrophic, or granular kidney in its most elementary lesion, of course in an incipient form. If this be anything more than a mere accidental observation, if it is, as I believe, a statement of fact from which you can generalise at all, we are bound to assume that in a certain proportion of cases of scarlet fever (and in cases which end in death probably in a very large proportion) you will find the beginnings of interstitial nephritis—the form which has been considered the most hopeless of all. I believe that Dr. Coats will bear me out in saying that he has found this not only in scarlet fever, but in one or two cases of acute inflammatory nephritis, some of which occurred to myself. These and many other facts tend to make us pause in recognising to the full a complete subdivision, such as has been usual of late, of Bright's disease into entirely separate species or varieties. It seems to be perfectly clear that while there are types and general methods of invasion and progress, and also certain typical morbid appearances as a final result of long standing albuminuria in Bright's disease, you cannot lay down the law that there are three or four kinds of disease that keep entirely separate in fact, as regards their origin and progress to a fatal result.

Now, then, comes the question, Is there not in Bright's disease an underlying unity, some circumstance in common to all these varieties; which, when we are able to get at the true pathology of it, will really be the essence of Bright's disease? Here I must refer to Dr. Mahomed, who seems to me to have given by his most laborious, valuable, and fruitful researches, quite a new start to this whole subject. He has accumulated so many new facts and reasonings that they almost take one's breath away in the meantime, and prevent one from assuming that the pathology of the present day is to be that of the future; but as Dr. Mahomed is to follow me this evening I shall not attempt to anticipate the statement of his views on the subject. I would say with regard to his papers, however, which I have read with the greatest attention and the greatest interest, that one point in them struck me very much as in harmony with my own ideas. What I, for one, have constantly taught, in accordance with some of Dr. Mahomed's views of Bright's disease, is the great importance to be attached not merely to the fact of albuminuria or no albuminuria—for the purport of Dr. Mahomed's researches is to bring albuminuria into a quite secondary position—but to the question whether the kidney is actually doing well its real functional work; whether it is "adequate" (as Sir Andrew Clark puts it) to the work of eliminating the waste nitrogenous matter of the tissues—the urea and other physiological elements of the urinary excretion. And this is to be discovered only, if at all, by using the specific gravity and whole quantity of the urine taken together, over considerable periods of time, or otherwise by daily testings of the amount of urea in twenty-four hours (a rather troublesome process to be carried on from day to day, and from week to week) as a test of whether Bright's disease is to be considered curable in any particular case or not. The particular form in which he puts the observation upon that point—namely, to multiply the last two figures of the specific gravity into the daily quantity of urine—not as an exact statement in grains or any other measure of the solids of urine, but as a sort of proportional statement, seems to me a very convenient formula indeed, if only it can be relied upon as being moderately accurate.

Passing from this to a somewhat more general view of the subject—in my own earlier experience of pathological investigation, I used to liken renal albuminuria to pulmonary catarrh —not only in respect of its being a transudation of blood serum, but in respect of its leading up to an indefinitely numerous series of varying lesions of the organ chiefly affected, as well as of other organs in the same individual. In the case of catarrh of the ultimate bronchi we know that the individual becomes indefinitely liable, if his catarrh becomes habitual or chronic, to collapse of a certain proportion of the pulmonary lobules, or groups of air cells, and thereafter to lobular atrophy, to emphysema, to broncho-pneumonia, to bronchial abscesses or *vacuoles*, to various forms of excavation, tubercular and non-tubercular, to cirrhosis of the lung, and again, as a yet more remote consequence of these various pulmonary lesions, he becomes liable to disease of the right side of the heart and disease of the liver and kidney among others. It seems to me even now (and I must here say that I think I can speak at this time of day with entire freedom from prejudice of my own very early notions) that this ideal parallel between albuminuria and pulmonary catarrh is not at all a bad idea; and although analogies in medicine, as in other inexact sciences, are proverbially unsafe, I think, subject to necessary deductions, this is a very good analogy.

The first process of which we have a clear cognisance as a symptom—the catarrh of the mucous membrane in the one case, the transudation of albumen in the other—is in a certain sense the starting point of numerous lesions; the initiatory process upon which a great number of organic changes may depend, according to the proclivities of the individual, according to his idiosyncrasy, according to the surroundings in which he lives, and to a number of other circumstances. Without maintaining this view as strictly in accordance with modern research, subsequent speakers will perhaps tell us how far it is in conformity with their ideas and experience. I think that, clinically speaking, there is a good deal to be said for it. Both albuminuria and pulmonary catarrh may be mild or acute, transitory or chronic; simple, and of a good prognosis on the one hand, or irreparable in its consequences on the other, because associated with disorganising changes in a great variety of organs, which in the very nature of them cannot be repaired. According as our field of observation ranges chiefly over the simpler and milder, or on the other hand over the more grave cases, or according as we take pains to widen our clinical field by not paying exclusive regard to cases that occur amid grave conditions and end in *post mortem* examination, we shall be able to indulge in the more or less favourable prognosis of albuminuria in general.

Probably the current of ideas for the last half century has been towards a too grave prognosis on the whole, but with all that, it is a long way from this general statement of diminishing gravity of prognosis of albuminuria in general to Dr. Roberts' proposition of "albuminuria in health," and still more to the proposition of a "physiological albuminuria." I don't dispute my friend Dr. Roberts' facts at all. He is too careful an observer and too faithful a recorder to state anything that is not absolutely to be trusted on this score. I don't dispute his facts, but I don't think we can in the present state of our information accept the expression of "physiological albuminuria" without some demur, and perhaps some alteration of its terms. We may, indeed, admit that albuminuria may occur more or less frequently in persons who, to all appearance, are healthy—nay, it may persist in such persons, and may even become habitual. Does this fact entitle us to call such albuminuria "physiological?" I have good reason to know that a loud and not hæmic cardiac murmur may exist in association with apparently very good health. I am personally and professionally acquainted, and have been so for a quarter of a century or more, with at least one man who has had such a cardiac murmur for years, possibly, indeed, congenitally (for I myself examined him carefully in his boyhood); and this gentleman has not only been twice married and brought up two families of children, but has insured his life, and at this moment he professes to be a perfectly healthy man, and is to all appearance a very healthy man, apart from the existence of this murmur. Shall we on this account maintain that this is a case of physiological cardiac murmur or a cardiac murmur of health? I had last summer a little girl under my care in hospital for a considerable time, of whom I am able to affirm with confidence that for months together she has had a very distinct crepitus over the right lung, constantly to be heard ever since it was first heard, yet she was all along fat and ruddy, gaining constantly in weight during my whole period of observation of her, absolutely free from fever, as tested by thermometric observation both by day and night, and, I should say, generally speaking, in perfectly good health; and she had had, in the early part of her treatment, a thickening of the omentum, but that did not apparently concern or injure her health at all. Are you to say that these lesions are physiological, or belong to a state of health? I don't think we can quite admit that. I could give you numerous instances of the same kind.

We frequently meet with hæmorrhages—sometimes rather copious and distinctly abnormal hæmorrhages—from the nasal, pulmonary, uterine, hæmorrhoidal vessels, in persons who are said to be, and who really believe themselves to be, in perfectly good health; but we do not call these hæmorrhages on this account "physiological." The practical test of all that I think would be to put to ourselves this question—Would any one of us recommend for insurance, without an additional premium, a person in whom, under any circumstances other than the most obviously transitory and accidental, albumen had been found to be present in the urine? I am speaking, of course, of albuminuria as detected by the ordinary tests, and apart from special refinements of laboratory procedure. If albuminuria, in a practical and clinical sense, exists, and goes on for weeks or months, is there any man in this room who would consider himself authorised, on any amount of declaration set forth, to commend such a case for insurance without an additional premium? I think you will at once see that it is premature in this sense to speak of physiological albuminuria.

I had, two or three years ago, a case which, although not perhaps in accordance with the precise idea of "physiological albuminuria," may be mentioned here incidentally. A lady was brought to me for insurance, stating herself to be in good health, and, as far as one could judge from appearances and from the ordinary questions, really in good average health; but she had had scarlet fever some months before. I, as a matter of course, tested her urine, though otherwise there was no special reason for doing so, and found it very highly albuminous. There had never been any dropsy.

The urine was perfectly clear, and absolutely normal otherwise; it was of good specific gravity, and there was no sediment at all, even to the microscope. I kept that lady under my eye for two years, examining her urine from time to time, and found it always largely albuminous. Is there any one who would have pronounced that woman perfectly healthy, or who would have recommended her for insurance without an additional premium? I think the general sense would be that you could not do so consistently with proper regard to the risk incurred in insuring such a life; and, as long as this is so, I don't think we can speak about a strictly physiological albuminuria. It is, I fully admit, a question open to further consideration how far very minute amounts of albumen, or albumen not cognisable by the ordinary tests, may exist in the urine as a strictly physiological occurrence. I consider that albuminuria, in a practical sense, is, to say the very least, a danger signal, and a danger signal of the first importance. It may consist with apparent health; it may even, for aught I know, consist with long life in certain particular cases. So, too, the danger signals may be disregarded in a particular line of railway for a time, and yet no train may go into collision; and in like manner albuminuria may be there and the person may live on nevertheless; but a life under these circumstances is always subject to increased risk, and in all probability that state of matters cannot be regarded as physiological.

Physiological Albuminuria: Albuminuria Associated with Nervous Affections.

By DR. MORTIMER GRANVILLE.

Mr. President and Gentlemen,—The few remarks with which I shall venture to trouble you will chiefly bear on the second division of your subject, namely, the "Clinical significance" of albuminuria, although I may have a word—and it shall be only a word—to say about what I should like to call the *physiological-pathology* of the presence of albumen in easily discoverable quantity (I am careful to say easily discoverable quantity, because I feel sure there must be always some albumen present) in the urine. While it is now admitted that there is such a thing as "physiological albuminuria," clinical experience, as commonly interpreted, is held to show that a *pathological* state of function, generally if not always, exists when albumen finds its way, with the other excreta composing the urine, as far as the ureter, although there may be nothing amiss with the apparatus of urinary secretion. We have, by usage, so linked this term pathological with the idea of morbid anatomy, that it is, unfortunately, difficult to convince ourselves that there can be very considerable and even long persistent disorder of function without organic disease. How difficult, for example, do we find it to believe, even on the evidence of unquestionable facts in clinical and *post mortem* pathology, that locomotor ataxia, with all its characteristic symptoms typically developed, may exist without tabes or sclerosis, and purely as a disease or disturbance of peripheral origin ?

If the important researches of Leube and Saundby, having for their object to determine whether albumen—even in easily discoverable quantities—is not frequently present in the urine of healthy persons, and also in healthy urine—which is a necessary distinction—were carried still further it would, I believe, be found that so commonly does serum-albumen escape from the blood-vessels in the kidneys and find its way into the bladder, that only when collateral evidence of organic disease is forthcoming should "albuminuria"—except in its grave and typical developments—be regarded as a serious symptom. As practitioners of the art of healing, we are not brought into very close contact with those who have no need of a physician. We are not in the habit of testing the urine of our friends, and I hope we do not often experiment on our own. The inquiry—which really ought to precede such a discussion as this—namely, To what extent is albuminuria a frequent accident of health ? would need to be conducted in a fashion very different from that in which Leube, or that in which Saundby, have prosecuted their researches. It is not enough to test the urine of out-patients at an hospital, for it is fair to presume that these persons have *something* the matter with them ; nor will it suffice to make a few scientific raids on the urinary excreta of a regiment. Soldiers, for the most part, in times of peace, lead monotonous, though perhaps dissipated, lives, and there is a sameness in their food to which the organism, probably, accommodates its habits and methods of function. What we want is a careful and unprejudiced observation of the urine of persons of all classes and habits of life ; not to detect kidney disease, but simply to ascertain the facts about albumen in the water, and—which is of the highest moment—an observation which shall extend over a lengthened period for the urine of the same individuals. The very little

I have been able to do in this way leads me to think that just as you may look for filariæ in the blood of a filarious subject every day and never find them, so you may examine the urine of a score of healthy persons occasionally, and repeatedly, without any result; whereas, if the obvious precaution of collecting the urine for twenty-four hours be taken, albumen may be found—though, of course, in much smaller proportions than the percentage would show if it chanced to be tested at the particular period when the escape actually takes place. Whether the presence of albumen in the urine when voided be due to excessive filtration under excessive blood pressure, or to the failure of some normal function, *e. g.*, by defect of the supposed organic agents in the hypothetical process of re-absorbing the albumen, it must, for the purposes of the present discussion at least, be assumed that there is something amiss in the manner in which the work of preparing the urine in the kidney is performed, although, as we have said, there may be nothing wrong in the apparatus of urinary excretion. Starting from this position, I wish to submit for your approval a proposition, a plea, and a protest.

You have already been reminded of certain facts with regard to the influence of the nerves on the kidney, or, to speak broadly, on the excretion of the urine. For example, that " section of the renal, or of the splanchnic nerves, produces an increase in the secretion ; that section of the cord below the medulla leads to a great general vascular dilatation, a consequent fall in the blood pressure of the glomeruli, and a diminution in the quantity of urine secreted ; that stimulation of the cord below the medulla, although producing exactly the opposite result—viz., general vascular constriction, does not cause an increase in the flow of urine so long as the nerves of the kidney are in action, but if they be divided, the walls of the renal arteries become relaxed, the blood pressure in the glomeruli and the volume of the blood passing through the kidney are consequently increased, and a copious flow of urine is the result." From this it has been concluded, " it is, therefore, evident that constriction of the arteries, or arterioles, of the body, so long as the vessels of the kidney are not involved, causes an increase in the flow of urine ; but if they also be contracted, the pressure of blood in the glomeruli will probably be reduced, and the secretion of the urine diminished." It is also within your recollection that Müller and Peipers have shown how the flow of urine may be arrested—though not always immediately—by the division or destruction of the nerves going to the kidney. Thus, when a ligature is applied tightly around the vessels and nerves entering at the hilum, including everything except the ureter, and the ligature is presently cast loose, leaving the vessels once more patent and comparatively uninjured, while the nerves are crushed, the flow of urine soon ceases, and, which is important, the nutrition of the tissues composing the organ is so perverted that it becomes disintegrated. Now, these general facts relating to the innervation, taken in connection with the well known phenomena of polyuria and sugar excretion, with or without polyuria, produced by excitation of certain points or centres in the floor of the fourth ventricle, and the familiar experience of a direct influence exerted on the kidney by the cerebrum through vasomotor fibres in the sympathetic, causing either an increased flow of urine, or simply a more frequent desire for micturition, or both together, under certain forms of "nervousness," will, I believe, and submit, be found on further inquiry to supply an amply sufficient explanation of many, if not all, of the phenomena of albuminuria in its simpler, and even in *some* of its graver forms. I do not forget that, as you have been reminded, Cohnheim and other German pathologists have denied that albuminuria has ever been produced by section of the splanchnics, or even by division of the renal branches themselves; and I have in mind the fact that you have been asked to regard the experiments of Overbeck—or recorded by Overbeck—and Hermann as "utterly worthless" as an argument, that diminution of blood pressure following upon increased tension may be one cause of albuminuria; and I fully appreciate the force of the objection urged against the value of experiments made with a compound membrane, and yet I ask you not to be too precipitate in casting aside the hypothesis of albumen extrusion by pressure produced by vasomotor influences.

I will now beg your permission to mention two classes of cases of albuminuria which fall under the observation, especially—perhaps almost exclusively—of practitioners among the subjects of nerve-trouble and the "nervous." As a matter of fact, I have reason to believe the presence of albumen in the urine of patients of the classes I am about to indicate is not generally recognised. This is why I am desirous of calling attention to the matter; and further, because I believe that what happens to these persons may happen to others who are practically healthy.

1. A young man, whose genito-spinal centre has been unduly excited, as often by fighting against his passions as by giving way to them—the cerebro-spinal irritation being

not less in one case than in the other—falls into a miserable condition of mental, and a feeble state of bodily, health. He complains, among other ills, of pain in the back, not over the spine, but over the region of the kidney, generally on one side only or chiefly, and preferably the right. There is pain on pressure, the pain of tenderness, as commonly happens in neuralgic affections of the viscera when complicated with vasomotor depression and consequent atonic congestion, the supposed diagnostic value of "pain on pressure" notwithstanding. The sufferer goes to a physician: not for albuminuria, of course. Of this he knows nothing, and in a proportion of instances the practitioner knows nothing of it either. He does not examine the water, because he probably regards the case as one of "backache," from masturbation, which it is not; or, if he does suspect anything, it is "kidney disease," and when he finds a trace of albumen he is confirmed in this diagnosis. I do not know which is the worst misfortune, that the physician should overlook an instructive scientific fact, or that he should misinterpret the clinical phenomenons and frighten his patient. The consequences of scaring young men and their friends with albuminuria are very terrible. Many a victim of this mistake leaves the consulting room of his inapt or unfortunate physician miserably ruined. It may seem that this is a strong expression; but I am not overstating the facts. I could show you such science-made wrecks. They would fill a terrible register. They, unhappily—with infinite pity and regret be it said—number among them too many students of medicine and promising young members of our profession. These are not cases of the "albuminuria of puberty," or "of adolescence." They may, or may not, be "intermittent" or "transitory." They are simply cases of vasomotor disturbance, producing first constriction and then atonic dilatation of the capillaries with either increase or diminution of the amount of urine excreted. The albumen is not always constantly present, but it is very commonly found, and may nearly always be detected, by heat and nitric acid, in the urine collected for twenty-four hours. The urea is either greatly in excess, reaching as much as 15 to 20 grs. per oz. instead of 8, or diminished, sometimes falling as low as 4 grs., according to varying conditions not easily formulated, but probably dependent on the general relations of assimilation and disassimilation throughout the body. The other type of albuminuria is one in which the patient is middle aged, and has been, or is still, intellectually active, but he suffers from intense "nervousness," with agoraphobia or claustrophobia, sometimes both. He, too, has albumen in his urine, but it is not generally either high in percentage or persistent, and it often alternates with sugar, thus pointing significantly to the fact that the seat of the disturbance is within the cranium, though in some cases I have found reason to think that the focus of the excitation lay at the cilio-spinal centre in the cervical region.

What I especially desire to bring before you is the fact that the albuminuria in these cases is simply and purely of nerve origin, and if disease of the kidney should ensue, which is a possible contingency, it will be the effect, and not the cause of the albuminuria, or rather the result of that disturbance of nutrition to which the nerve trouble has given rise. It is, I am convinced, an error in diagnosis to place the albuminuria in the front place in these cases, and the consequences of so doing are too commonly disastrous. The pathology of such cases I believe to be as follows:—Some cerebral or reflex excitation of frequent occurrence or long continuance has rendered the vasomotor centre, or a particular system or series of the vasomotor nerves, debilitated and preternaturally irritable, with the result of causing the coats of the vessels supplied by these nerves to become unduly lax in their structure and deficient in their tone. At first they contract spasmodically, and then they dilate excessively, so that they, and the afferent vessels of the Malpighian bodies, are readily congested. In fact, I see no reason why, under the influence of nervous depression and vasomotor disturbance, the kidney may not become œdematous as a hysterical joint. The persons affected as I have described are nearly always subject to flushings and pallors, to local perspirations and shiverings; they are either very emotional or reserved, and for the most part lead melancholy or studious and solitary lives. If you ask me why the renal vessels should be specially affected in such organisms, I reply, because the anatomical structure and the physiological function of these vessels combine to render them peculiarly prone to lose tone and become congested whenever the blood pressure generally is increased. The proposition I desire to lay down is this; that in the nerve disturbances and excitations, to which these patients are subject, are to be found sufficient causes of the albuminuria which occurs in their cases; and I further submit that the operation of the same causes, in a transitory way, in persons of average health, may and does produce like results. The plea I have to urge is that these cases of cerebro-spinal or nerve-albuminuria, differing as they do from cases of "physiological albuminuria," may be recognised and set apart. To this end members of the profession generally will do well to examine the urine of *all* their patients, both for albumen and for urea. The two tests ought always to go together, and the precaution should be taken of collecting the urine for twenty-four hours, neither more nor less, so that the proper number of meals may be included. Note should also be made of special excesses in diet, particularly excesses in the use of any one kind of food.

The protest I feel bound to make, and it is, from my point of view, at least a very serious one, is that by confounding such cases as I have attempted to indicate with the ordinary class of cases of albuminuria dependent on kidney disease, men and women are mentally depressed and distressed, and left to drag out cheerless and useless lives. I have said that I have seen young men with nothing really serious the matter with them ruined by a misconstruction of the fact of a trace of albumen found in the urine. I have seen women distressed beyond measure, and in their dejection so distraught that they have been with difficulty prevented from destroying themselves. I have, at this moment, in my recollection, the case of a well educated woman, the mother of a family, who was reduced to this extremity, and placed in this jeopardy, by the discovery of a little sugar in her urine! Let me plead for a more general and a wiser use of the urinary-test-case. If we do not take pains to prevent misconception in our own minds, it is vain to hope we can prevent the needless alarm of our patients. It is not necessary that the physician should tell what he finds when he discovers albumen, his face and his prescription reveal the fact. We live in a day when a little learning in physiology and medicine is only too likely to drive the nervous patient mad. The area of probable error and the possibilities of mischief have been so largely increased of late, for example, by the introduction of such untrustworthy tests as picric acid and the like, that I felt bound to take advantage of this almost unique opportunity of bringing the matter under the serious consideration of the profession, and of raising a warning voice against what I believe to be one of the worst abuses of the scientific method, which the progress of medical science in this or any other age has witnessed, and which it is as necessary to expose as it is impossible not to deplore.

On Albuminuria and the Symptoms which indicate its Gravity.

By Dr. MAHOMED.

Dr. Mahomed read abstracts from a paper treating the whole subject of albuminuria at considerable length. The entire paper will appear in the next volume of the *Guy's Hospital Reports,* for which it was originally prepared. The leading points of the paper are here mentioned :—

It was stated that the following experimental results had all been well ascertained and were generally accepted by the leading authorities :—1. That albuminuria can be produced by ligature of the renal veins. 2. That albuminuria may result from injecting egg albumen or peptones into the blood. 3. That albuminuria is produced by exudation from the Malpighian glomerules; this was proved by the experiments of Nussbaum, and also by others of a different nature by Ribbert, Posner, and Litten, who also proved that albumen was not found in the glomerules or tubules of the healthy kidney. 4. That albuminuria might be produced through the nervous system by irritating the spinal cord after division of the vasomotor nerves of the renal arteries; by other direct or reflex disturbance of the nerves controlling the circulation through the kidney. 5. Albuminuria could be produced by cutaneous asphyxia. 6. By the disordered nutrition of the walls of the blood-vessels, tubules, and epithelium. 7. The experiments on the production of albuminuria by increasing the arterial pressure were discussed at length, and the difficulty of raising arterial pressure by compression of the aorta was demonstrated by experiments performed by the author in 1875 (tracings of blood pressure were shown). The fallacies of several experiments on this subject were pointed out, while the evidence of Dr. Michael Foster on the production of albuminuria by increased arterial pressure, when the renal arteries were dilated, was quoted. The inability of the physiologists to produce variations of arterial pressure by retarding or accelerating the capillary circulation was pointed out. A suggestion was made that investigations into the variation of the tension and the rapidity of the interchanges of the gases in the blood and tissues were urgently needed. The theory propounded by Professor Hamilton that the increase of arterial pressure was due to the diminished specific gravity of the serum was criticised, and it was pointed out that the specific gravity of the serum was not usually decreased to any considerable extent in Bright's disease; the observations of Quincke were quoted in support of this statement. The sp. gr. having been observed only in cases of long standing disease, the alteration was probably very gradually induced; on the other hand, it was shown that in order to satisfactorily account for the increased arterial pressure of Bright's disease it was necessary that the alterations in specific gravity should be very rapid, that they should occur in a few hours. A belief in such a rapid change of gravity was contrary to the results of all previous physiological experiments on the subject. The observations of Runeberg on dialysis through compound membranes were condemned as incorrect and misleading.

The evidence afforded by clinical observation and morbid anatomy concerning the causation of albuminuria was passed in review, and classified under the following headings:—

1. Albuminuria produced by obstruction to the venous circulation; under this head was included the albuminuria due to heart disease and similar conditions, to the enfeebled and retarded venous flow in fever, anæmia, and conditions of extreme prostration.

2. Albuminuria produced by errors of diet or digestion, such as that produced by a diet largely consisting of white of egg (hæmatogenous albuminuria).

3. The influence of the nervous system was alluded to, and the observations of Laycock, Fürbringer, and Sir Andrew Clark were quoted.

4. The intimate association between the skin and the bowels was demonstrated by the author's previous paper on scarlatinal albuminuria. The cases of albuminuria produced by inunction for scabies, recorded by Lassar and Unna were quoted.

5. Alterations in the tissue of the blood-vessels, tube walls, and epithelium were held to be sufficient causes of albuminuria in acute nephritis. Some observations on the morbid histological appearances of the kidney were made; it was stated that the so-called glomerular nephritis was not confined to cases of scarlatina; the unity of Bright's disease was affirmed.

6. The previous observations of the author showing the close relation between increased arterial pressure and albuminuria in scarlatinal nephritis, and other more chronic conditions, including pregnancy, were described.

The rapidity of the circulation of the blood in the normal

Malpighian glomerules, owing to their immediate connection with large arteries, and also to the large size of their capillaries, was dwelt upon; it was pointed out that the circulation in them was probably more rapid than in any other capillaries in the body, and conclusions were drawn from the well ascertained facts previously enumerated that *retardation of the glomerular circulation*, combined with increased pressure in and increased distension of the glomerular vessels, were the chief factors in the causation of albuminuria; it was pointed out that while water might readily transude from the glomerules under normal pressure, albumen being less diffusible would probably be retained, but that slight delay and increased turgescence in the glomerules might permit of the exudation of paraglobulin and other more diffusible forms of albumen, and subsequently, if the obstruction increased, of serum albumen. Cases were subsequently quoted indicating this.

The immediate causes of albuminuria were classified as follows:—

1. Increased glomerular pressure and slowing of the blood stream in the glomerules:—

   a. From general increase of arterial pressure with local hyperæmia of the kidney, due to disorder of other excretory organs, especially of the skin and bowels. This may occur as a functional condition.

   b. From general increase of arterial pressure, aided by the augmented distension of the glomeruli by obstruction of tubular plexus, either by expansion of tubules by proliferated epithelium, or to compression of plexus by fibroid growth. This condition is necessarily due to organic changes.

   c. From increase of arterial pressure in the kidney, due to derangement of the nervous system, either local, general, or reflex. This may be purely functional, or may accompany organic changes.

   d. From obstruction to the return of the venous blood. This may be due to a functional condition or to organic changes in other organs.

2. From alteration in the tissue of the blood-vessels, tube walls, and epithelium, produced by acute inflammation or infarction.

3. From chemical changes in the blood, especially the presence of abnormal forms of albumen in the blood serum.

The question of what is meant by albuminuria was then considered. It was stated that a very large variety of albuminous bodies were found in the urine, and that the form of albumen present in any particular urine was probably constantly undergoing changes from the moment it was secreted until many days afterwards. It was stated that the same specimen of urine frequently gave different reactions with the same tests at different intervals of time after it was passed. It was shown by many examples that heat and nitric acid each failed to detect the presence of albumen in the urine in some specimens, that they did not precipitate peptones, or that form of albumen which was mistaken for them, and which the author believed to be paraglobulin; it is well known that they often fail to detect many forms of acid and alkali-albumen, including syntonin and myosin. For these reasons the use of such tests as the brine test, picric acid, and potassic-mercuric-iodide were advocated. It was stated that these were exceedingly delicate and good *negative* tests, but that their results required to be checked by other tests, such as the ferrocyanic pellets, which did not precipitate peptones. The fallacies of a large number of tests were then pointed out, and their respective advantages were indicated. A small and convenient pocket case, containing only dry tests, and combining Dr. Pavy's and Dr. Johnson's pocket tests, with other necessary apparatus, was exhibited.*

It was shown that many cases occurred in which albumen detected by nitric acid and heat alternated with a form of albumen only recognised by the more delicate tests, especially brine and picric acid. The albuminuria occurring in apparently healthy persons was considered. The author stated that he had found albumen in the urine in *nine* out of *fifty-eight* persons (namely in 15·5 per cent) thought to be healthy, whom he had examined previous to life insurance during the past year. Details of the cases in which albumen was found were given, and it was shown that seven out of the nine cases were clearly not in perfect health; it was suggested that many such cases were produced by temporary functional disturbance.

Albuminuria was divided for diagnostic purposes into several groups:—

1. Cases of fully developed Bright's disease.

2. Cases with the cardio-vascular symptoms and changes of Bright's disease, but with only slight or occasional albuminuria. These belonged to the class described by the author in a previous paper as cases of "Bright's disease without albuminuria." They presented very varied aspects.

* Made by Messrs. Down Brothers, St. Thomas's Street, S.E.

3. Cases in which the albuminuria was considerable and prolonged. These were divided as follows—

  a. Those with no other symptom of Bright's disease; the arterial pressure not being increased. The quantity of albumen being small. Prognosis good.

  b. Those without increase of arterial pressure, but with occasional œdema. The quantity of albumen being large. Prognosis hopeful.

  c. Cases in which the albuminuria is persistent, and is associated with increased arterial pressure. Prognosis grave, but never hopeless.

4. Cases of temporary and intermitting albuminuria associated with anæmia, debility, or some other condition which can be easily recognised.

Several cases, with sphygmographic tracings, illustrating these groups were related.

It was pointed out that Bright's disease had two distinct aspects—the renal and the cardio-vascular, and that either of these might be present for many years without producing any symptoms or causing any anxiety; the existence of these symptoms merely tells of possible danger in the future. If both albuminuria and high arterial pressures exist in the same case the cause for anxiety is greatly increased. The chief advantage of recognising the condition is the key it gives us to the general management and treatment of the case, and of the most effective way to deal with the intercurrent ailments which may from time to time arise.

In conclusion, Dr. Mahomed wished to briefly state his own views concerning Bright's disease:—The first step is the existence of an acute or chronic form of blood poisoning, either due to some definite poison, such as scarlatina, lead, or alcohol, or to the blood being laden with an undue quantity of excrementitious material. This produces two important results, a rise of arterial pressure from impeded capillary circulation and a more or less severe functional hyperæmia of the kidney, this being the chief excretory organ of the body; the other excretory organs—namely, the lungs, gastro-intestinal mucous membrane and the skin being liable to be more or less affected. The recognition of this condition led Dr. Mahomed to describe the condition, ten years ago, as one of chronic or acute excretory congestion. When this condition exists, either a chill, or constipation, or an excessive amount of the poison may increase the hyperæmia to a condition of more or less severe inflammation, and this gives rise to albuminuria by further impeding the circulation through the glomerules. If, however, no inflammatory congestion takes place and the condition is a chronic one, albuminuria may be only occasionally present, and that in small amount; but the cardio-vascular changes, produced by high arterial pressure, develop—the vessels thicken, and the heart hypertrophies. The glomerular capsules also thicken and exert rather a protective influence over the glomerular vessels, assisting to prevent their over-distension, and to obviate the tendency to albuminuria. On the other hand, the changes in the capsules may advance too quickly till they strangle the vessels they contain, or the inter-tubular changes may be considerable, and by compressing the tubular plexus may induce albuminuria. Thus, the renal symptoms liable to present themselves depend upon the severity and degree of the changes that occur in the kidneys.

The Clinical Significance of Albuminuria: Influence of Toxic Agencies.

By Dr. JAMES FINLAYSON.

Mr. President and Gentlemen,—When it was suggested that I should take part in this discussion, I felt that the only subject I was entitled to speak on was the clinical significance of albuminuria. Now, the first thing is to indicate what I mean, when I speak to-night of albuminuria, for the term has been used in this discussion in various senses. I mean to include all those cases where we have good grounds for believing that the urine as secreted by the kidney is albuminous, and the tests for albumen on which I have relied are the two so well known and so long tried—the heat test, and nitric acid in the cold. Not that I have not used other tests, including the picric test, of which we have heard so much of late. Ten years ago I was in search of a test for the quantitative estimation of albumen, and my attention was directed to a paper by Esbach,* where he used picric acid for this purpose, and I now show you an instrument which I procured from Paris at that time, which many of my old pupils may recognise; but I soon found that picric acid, while apparently an admirable *negative* test, as Dr. Mahomed says, gave at times precipitates which I was sure, from clinical experience, were not due to the albumen I was in search of. At the time the circumstances were not

* *Bulletin Général de Thérapeutique*, 1874.

favourable for investigating the nature of these precipitates, and I was in search, as I said, of a quantitative test, which was afterwards supplied by the dilution method introduced by my friend Dr. William Roberts, and so I did not pursue this branch of inquiry. I do not say that these tests should not be employed; but I perfectly agree with Dr. Roberts in saying that they should, for a long time, be relegated to the laboratory, to be employed by men like himself or Dr. Mahomed, or others who can be trusted to check their observations by other methods.

I do not deny, indeed if need were I would contend, that albuminuria may result from changes as regards pressure, particularly of venous pressure in connection with grave obstruction from cardiac and pulmonary disease. I likewise admit that pressure from within the urinary passages may lead to renal disease and albuminuria,—indeed, the last case I showed to this Society was one of chronic distension of the bladder, leading apparently to interstitial nephritis. I have it likewise in my notes here that disturbances of the nervous centres may lead to albuminuria—a subject just insisted on by Dr. Mortimer Granville. I am equally prepared to admit that we may have albuminuria at times in healthy persons, as contended by Dr. Roberts at last meeting: some few cases of this kind, occurring in substantially healthy persons, have come under my notice; but when persistent, I cannot shut my eyes to the fact that we may be in presence, in such cases, of an incipient granular degeneration of the kidney.

But when viewed broadly, albuminuria seems to me to be oftenest due to the influence of poisons operating on the system, or at least to the presence of grave constitutional disturbance having many points of resemblance to that induced by acute or chronic poisoning.

Various poisons are known to give rise to albuminuria, and if need were I could enumerate a long list from the text books; but I will take as a *type of acute poisoning*, Cantharides; and of *chronic poisoning*, Lead; while intermediate between the acute and chronic forms, and combining both, we have the well known poison Alcohol.

The acute type is well represented by cantharides. I had just lately the opportunity of observing albuminuria developed in a girl from the application of a blister for a pleuritic attack. Her urine previously was free from albumen, but after the blister, albuminuria (without blood or other signs of strangury), supervened for a few days, and in the urinary sediment we found tube casts and other evidences of a renal affection. Now, in searching for conditions resembling acute poisons, we are forced to think of the great group of infectious fevers. In October, 1876, I had the opportunity of examining the urine of a girl affected with malignant pustule or anthrax, whose case was subsequently brought before this Society by Dr. Hector C. Cameron. I found her urine highly albuminous; going upon the old rough method, there was a sediment of $\frac{2}{3}$ of the amount operated on after it had been boiled and allowed to settle for 12 hours. I found, likewise, tube casts in the sediment in abundance. I select this somewhat rare disease first, because its pathology has been determined more thoroughly than some others, through the labours of many distinguished men, amongst others Professor Greenfield, who has honoured us with his presence to-night. Their researches have shown that this disease is due to the presence in the blood of a multitude of specific organisms. But the whole group of the *specific fevers and exanthemata* are evidently closely allied to this disease as to their pathology, although many of them have not yet been so clearly demonstrated to b due to living organisms in the blood; we have small-pox typhus, enteric fever, scarlet fever, relapsing fever, measles, puerperal fever, erysipelas, diphtheria—all these very often present albuminuria in their height.

Coming to another group of diseases, we have *pneumonia, pleuro-pneumonia, and acute tuberculosis*, which often have this complication. Ever since I have taught clinical medicine, I have insisted on the close resemblance which pneumonia presents to the specific fevers, and I think of late a very general tendency of opinion exists in this direction throughout the country; and confessedly, we have at times epidemic or infectious pneumonia. The very name, pleuro-pneumonia, suggests to our minds the infectious disease in cattle, although, of course, I do not insist on this. Acute tuberculosis is claimed by the strong advocates of the bacillus tuberculosis as a fever due to a specific organism; but without requiring to advance this doctrine, we must all admit the very close resemblance which this disease presents to a specific fever.

Coming to *rheumatic fever and rheumatic pericarditis*, we are again in presence of some poisonous material in the system, or a condition allied thereto (some, indeed, assert the existence of a specific germ), and here also we often have albuminuria.

Now these cases of albuminuria are not explicable simply on the score of high fever and excited circulation, for we often find in diphtheria, for example, very distinct albuminuria without much fever, and, indeed, without much constitutional disturbance of any kind.

I come now to *scarlet fever, erysipelas, and small-pox.* In these diseases we have already seen that we often have albuminuria at their height, but we often also have albuminuria as a *sequela;* with regard to scarlet fever this is admitted by all; with regard to erysipelas I think it is also admitted pretty generally, and, at least, I myself feel sure of it; with regard to small-pox, fortunately we have not often in Glasgow the chance of observing it, but in the epidemic occurring here ten years ago, Dr. Gemmell* studied the occurrence of this sequela of small-pox at the hospital here. Now all these diseases concur in affecting the skin in the height of the illness, and they also all affect the skin secondarily—scarlet fever and erysipelas by desquamation, and small-pox by the casting of the dried scabs. This combination suggests strongly the connection, forced on our attention in many ways as physicians, between the skin and the kidneys; and perhaps it may afford a clue to the connection, generally admitted, between the *influence of cold on the cutaneous surface and the occurrence of acute nephritis.* At any rate, the connection is suggestive, and one cannot keep out of view the old experiments of varnishing the skin of animals as possibly leading to the retention of noxious materials; in these experiments the animals, when the process is fully carried out, seem to die rapidly with low temperatures, but when less completely done, there are febrile symptoms developed, and along with these we have albuminuria.†

The next great group of cases to which I must refer depend on *pregnancy.* From the nature of my practice, I cannot, of course, speak of this matter from any great personal experience; but I never felt satisfied that the simple explanation of pressure met the facts of albuminuria in pregnancy, and it seemed particularly inapplicable for those cases occurring in the early months. Ten or fifteen years ago, my attention was directed to such cases, and I was much struck by the remarkable resemblances which pregnancy often presented to various acute and chronic diseases; indeed, I intended at one time to have pursued this inquiry from the literary and historical point of view, and so I read with great interest, when it appeared, the very able contribution of Dr. Barnes‡ on "Pregnancy regarded as an experiment illustrating general

* *Glasgow Medical Journal*, 1874.

† See e. g., *Carpenter's Physiology*, 4th ed., p. 632.

‡ *British Medical Journal*, 9th December, 1876.

pathology." In particular, I was arrested by the striking resemblances presented, in some cases I knew of, between the sequelæ and complications of pregnancy and the puerperal state and those of scarlatina in children. Now, in the pregnant woman we have an independent germ, so far, living and multiplying its cells with enormous rapidity, at the expense of the mother's blood, leading thus confessedly to very marked changes in the composition of the blood of pregnant women; and so we have a kind of comparison between this condition and the state of system we find in anthrax already referred to. But further, we know, at least it is so stated by good authorities, that the vitality of the fœtus has an important bearing on the occurrence of the signs of pregnancy, the tumid breasts, and the morning sickness disappearing with the death of the fœtus in utero. And I was much struck in reading the sheets of Dr. Leishman's third edition of his *System of Midwifery* (p. 260), to find a case occurring in the practice of Dr. Maclaren, where a lady, who had been affected with albuminuria from the 6th to the 8th month, suddenly became relieved of her dropsy and albuminuria, apparently at the very time the fœtus died, so far at least as could be judged by its appearance when expelled a week afterwards. This case, no doubt somewhat exceptional, seems to throw a distinct light on this interesting subject.

The *chronic type* of poisoning leading to albuminuria is well represented by the poison of lead. We in Glasgow know but little of this from personal observation; at least I speak for myself, and if I am wrong, or if my experience is exceptional, you, Sir, or some of the other physicians in our hospitals will no doubt correct my error. But the influence of lead in producing albuminuria is certified by so many and by such eminent authorities in London, that we may at once accept it as a fact. Probably the explanation of our knowing but little of this connection is this, that both lead poisoning and gout in a distinct form, which combine to affect the kidney, are really very uncommon in our hospital patients here.

Amongst chronic diseases having resemblances to chronic poisoning we have, first of all, this same *gout*, which is so closely connected with albuminuria as to have given rise to the name of the "gouty kidney."

Then we have *rheumatism*, which is, so far, allied to gout. I have already spoken of acute rheumatism giving rise to albuminuria in its height, but I have long felt satisfied that it sometimes operated in a more chronic way, and I was glad to find on reading Dr. Dickinson's new edition in 1877 that he believes in this influence also.*

Next we have *syphilis.* Of the causation of albuminuria from this poison, apart from the amyloid degeneration, I cannot speak from any observations of my own; but the name syphilitic albuminuria and the fact also seem generally admitted in medicine.

In *jaundice* we have a poison circulating in the system, and here also we frequently have albuminuria; but as I showed to this Society in 1874, we may have *abundant renal tube casts* in the sediment of jaundiced urine, even when the ordinary tests fail to detect albumen, showing thus a special influence operating on the kidney in this disease.

In *diabetes* we have, towards the end, a pronounced form of poisoning, and here, too, we often have albuminuria leading up to the fatal termination.

The next group of cases complicated with albuminuria is that important class known as *scrofulous* or *phthisical.* I have long been impressed with the importance of this group of cases of albuminuria—occurring quite apart from pronounced amyloid degeneration—and I was glad to see that Dr. Theodore Williams called attention to them in his recent admirable papers in the *British Medical Journal.* Now, here again I do not wish to contend for any extreme views as regards the dependence of these diseases on specific germs, in which case the relationship to poison is at once manifest; but, in any view, we have certainly a grave constitutional disturbance which some have thought can bear this comparison.

We come now to the important subject of *alcohol* as causing albuminuria. With alcohol we may have acute poisoning,† as it were, from a short lived influence, leading to albuminuria. Or we may have sub-acute poisoning leading up to delirium tremens, and alcoholic convulsions, in which condition in the few cases occurring in my experience we usually have albuminuria. But we may also have albuminuria determined by alcohol in a more gradual and chronic manner. For I think all hospital physicians in Glasgow will be disposed to agree in acknowledging the influence of alcohol in determining Bright's disease. Dr. Dickinson, indeed, is disposed to deny this, or at least to refuse to admit it very freely and fully; and he discussed the question on the basis of the statistics of cirrhosis of the liver.

* Of course I do not refer to the Néphrite rhumatismale of Rayer, which is embolic.

† *Dictionnaire Encyclopédique des Sciences Médicales:* Art. "Alcoolisme."

It has occurred to me some years ago, as a very curious fact, that while in Glasgow we could not claim for our population an excessive care in the use of alcohol—rather indeed have we to admit the reverse—the occurrence of typical cirrhosis of the liver, while by no means a pathological rarity, is equally by no means a common disease as judged by its detection in the hospital dead house; and it is much less common in our wards, so far as we can guess, than it is in similar hospitals in London or Manchester. I have not searched the *post mortem* records, but I appeal to Dr. Coats as a pathologist if I am not substantially correct. Now, it has occurred to me that with our wet climate here, and with a large proportion of our population employed in out-door work, or at least in work which exposes them very much to the weather—in the shipbuilding yards, at the harbour, &c. (or to damp in the coal mines)—we may have the injurious influence of cold on the surface leading to an affection of the kidney rather than cirrhosis of the liver, from the influence of alcohol.

There only remains one great group of cases, viz., the *amyloid or lardaceous forms of kidney disease.* These are well known to be produced by three leading causes. Of (1) phthisis I have already spoken. In (2) chronic suppuration we have, according to Dr. Dickinson, who has studied this matter carefully, a deterioration of the blood from a deprivation of some of its constituents. I do not care to contend that this is very closely allied to the phenomena of poisoning, although, if time permitted, I could adduce some reasons in favour of such a view. In any case, we have the grave constitutional state to which I referred at the outset. (3) The third recognised cause of amyloid disease is the well known poison of syphilis.

In this rapid review of the subject, I think I have adduced enough to show the great importance of toxic agents in the production of albuminuria.

Session 1883-84.

Meeting VIII.—*26th February*, 1884.

The President, Prof. M'Call Anderson, *in the Chair.*

Albuminuria in its Relation to the Puerperal State.

By PROFESSOR LEISHMAN, M.D.

Hitherto, Mr. President, the discussion on this important subject has been confined almost exclusively to the broader and more general aspects of the case. Had it been the desire of those who have the management of the discussion that it should have continued in this groove, I for one should not have presumed to rise to address this meeting, for the very simple reason that for many years my attention having been diverted into other channels, I feel myself quite incompetent to speak on subjects involving what may be called modern pathological speculation. But, Sir, I think the subject is one in which those of us who are more or less specialists must naturally take a very considerable amount of interest. I can imagine how an oculist—our friend, Dr. Reid, for example—must look upon this subject with great interest, in its bearing on diseases of the eye; and, in respect of that branch of the profession which I may presume to say I represent here, I certainly cannot hesitate to assert that, in regard to obstetrics generally, there are very few subjects the consideration of which one would approach with more intense interest than this. In looking at it from this point of view, with the permission of the members of this Society, and in limiting any observations that I may make to the relations which the subject bears to obstetrics in general, I think I shall not have difficulty in pointing out—if indeed there was any doubt on the subject previously—that there are many points in obstetrics quite apart from pathological questions which have a very interesting bearing on this subject; and several of the points to which I would venture to call the attention of this meeting to-night have been suggested to my mind by the observations which have fallen from previous speakers on the general subject. I was struck by an expression that fell from Dr. Roberts, which was certainly new to me, of "physiological albuminuria," which seems, on the first blush at all events, to involve something of a paradox. But, if there is any truth in this, or if in the expression there is even an approach to the truth, I think nowhere will there be found confirmation of this idea more strongly expressed than in connection with obstetrics. Allow me in the first place very briefly to call the attention of the meeting to the peculiar circumstances of the pregnant state, as bearing particularly upon this subject of ours. In regard to the blood, it is well known that its composition is altered in a very remarkable degree in the course of a perfectly normal and natural pregnancy. We find that, throughout the pregnancy, but more particularly in the later months, there is a marked increase in the quantity of fibrine: there is a great diminution of the corpuscular and a corresponding increase of the watery elements of the blood. I should perhaps have added—and that is a point which I should not have overlooked—that there is also, at that time, a diminution in the quantity of albumen in the blood. Now, these facts are very significant, inasmuch as they have a bearing upon more than one theory that has been suggested in the course of this debate. You will observe that these changes of the blood involve conditions which under ordinary circumstances we would call pathological conditions; and, inasmuch as these conditions exist in every case of normal pregnancy, we cannot but look upon them as physiological phenomena, and here we have an illustration of the apparent paradox. We have pathological phenomena which we cannot describe otherwise than as strictly physiological. Now, that is the first point that seems to me to have a bearing upon certain statements that have been made in the course of the debate.

Professor Hamilton, in his remarks, propounded a bold and ingenious theory, that perhaps the cause of the changes that we find in the disease known as albuminuria, may depend upon something wrong in the specific gravity of the blood plasma. Here again we have, in the conditions of the blood in pregnancy, a marked divergence from the normal standard in the specific gravity of the blood plasma; so that, whatever weight is to be attached to his theory, it seems to have some illustration at all events from the considerations which I venture to lay before you. Then, again, we have a diminution of the quantity of albumen in the blood in pregnancy. That may possibly have some bearing on the presence of albumen in the urine.

If we turn again from this consideration of the composition of the blood and look a little further to the state of the circulation generally, we come to observe that there is a remarkable alteration in the condition of the heart. I think it is now some 25 years ago since Larcher first demonstrated the fact that there was, during pregnancy, and as a natural condition of pregnancy, a remarkable addition in bulk and weight to the heart, and that that increase in bulk and in weight was almost entirely due to an increase of the left ventricle. I am quite aware that the assertions which were made on this subject have been controverted to some extent, and that although they were confirmed by many of his brethren of the French pathological school, German pathologists chiefly took exception to many of his views and attempted to disprove them. However, I think if any one will take the trouble to go over the whole subject and consider it, although he may come to the conclusion that Larcher and others somewhat exaggerated the facts of the case when they said that the heart was, in a normal case of pregnancy, increased by one-fifth of its weight—although we may come to look upon that as somewhat of an exaggeration—I think the facts, as established in the present day, conclusively show that you have an enlarged left ventricle. Now look at this, along with the altered condition of the blood. You have a condition of the left ventricle which involves vascular tension. You have thus one of the conditions which Dr. Newman, as I understood him, assumes as a possible and probable cause of albuminuria. Then you have, as I have shown, that altered condition of the blood which Professor Hamilton assumed as a very probable cause of albuminuria. Need we wonder that, in a considerable number of cases of pregnancy, we discover albumen in the urine. It has long been a familiar fact that, in a large number of pregnant women—as many as 20 per cent—albumen, or traces of albumen, are found in the urine. Whether we are to call these cases "physiological albuminuria" or not we need not pause to consider; but I confess I was not prepared for the statement that Dr. Roberts made, that, in a percentage of cases, very little short of that which I have just stated, we find albuminuria present in healthy young persons of both sexes. If we turn now from the state of the left heart and look for a moment on the other side of the kidney, we find that pregnancy involves, in that direction also, another special condition which may, and which undoubtedly does, in some instances, lead to albuminuria. I have never attached much weight to the theory which sought to find in pressure on the renal veins *the* cause of puerperal albuminuria—for this reason, chiefly, that we find it often in abundance at a period long before pressure can come to be exercised on the renal veins, and that, though it may be admitted as a possible cause of the albuminuria in the later months of pregnancy, it cannot, I think, be looked upon as a very important factor in the causation of this disease: but here we have, you will observe again, not only on the one side of the kidney, but also on the other, conditions extremely likely—and physiological conditions too—to develop certain changes that Dr. Newman brought under our notice; and I say again, how can we wonder that we find albuminuria very frequently in pregnant women?

I think there is no one in this room who will dispute the fact, which is consistent with the experience of all of us, that when we encounter in practice a case of albuminuria in pregnancy, we do not attach to the fact itself—to the presence of the albumen—the great importance that we would attach to it if we looked at the phenomenon as ordinary physicians in an ordinary case. Experience tells us that, although we have, for example, evidence of large quantities of albumen, in most of these cases the albumen disappears after delivery, or even after the death of the fœtus *in utero*—as in a remarkable case which I have cited elsewhere, and which has already been referred to by Dr. Finlayson—and the patient may never have manifested any symptom that would cause the slightest alarm; whereas, if the same amount of albumen were to be found in the urine in any other case, which you may call a medical case, we would look upon it with considerable apprehension. I think I am justified in saying that that is the experience of every practitioner who has had anything to do with the practice of midwifery.

Now I shall venture to go only one step further, Mr. President, in asking this meeting to consider for a moment the clinical significance of albuminuria in pregnancy, in its bearing upon puerperal convulsions. That, you may think, is a little wide of the question at issue, but it is a point of such interest to any one who has had to do with the practice of obstetrics, that I shall not apologise for calling your attention to it, and I shall take care to do so very briefly. In a case of albuminuria occurring during pregnancy, our dread as to the ultimate results has reference, I think, chiefly to the possibility of the occurrence, sooner or later, of puerperal convulsions. In a considerable number of cases puerperal convulsions never occur, but in other cases they do occur, and are looked upon by all of us as among the most serious catastrophes that can happen to us in the course of our practice. Many years ago a theory was started to account for the phenomena of convulsions as connected with albuminuria, which was brought under the notice of the profession prominently by Braun, and which attracted very considerable attention in this country as well as in Germany. I think I may say that, at the time it was first propounded, it was very generally accepted as a thing which was almost demonstrated. Braun's theory amounts simply to this, that the phenomena of puerperal convulsions are due to uræmic poisoning, as the indirect result of kidney changes—the accompaniment of albuminuria—that the poison in the blood, acting upon the nervous centres, causes these convulsive phenomena. He assumed—he imagined indeed that he had proved—that the poison which acted upon the blood was not urea, but carbonate of ammonia, the product of the decomposition of urea, and that this caused puerperal convulsions. Now, I think the reason why this theory attracted so much attention, and was so generally accepted, was that it is one which is easily understood, and besides that, that it was a theory which pointed in a very significant way to a method of treatment—a peculiarity of pathological theories which does not always exist, I am sorry to say. It pointed significantly to this, that if the blood was in a highly alkaline condition, acids should be administered—vegetable acids, citric, acetic, tartaric, and, above all, benzoic acid—and these acids were largely administered with the view of warding off the danger of puerperal convulsions, and my impression is strong—even at the present day, when that theory has been lost sight of to a considerable extent—that, whether the theory is a true one or not, in some way or other these acids do produce a beneficial effect. I don't know whether I am entitled to assume that this theory of Braun's is an exploded theory. It has gone out of view very much. It has, in fact, been lost sight of, and it has been lost sight of chiefly in consequence of a very brilliant theory which is known as the Traube-Rosenstein theory, in which it is assumed, as I understand it, that there is no poison, strictly speaking, in the blood, but that the phenomena are due to increased vascular tension acting on the brain, and the way in which this increased tension acts, according to these observers, is that there is, in the first place, an effusion of serum from the blood into the brain tissue, and that the presence of the serum, which is thus effused from the capillaries, constitutes œdema of the brain. The serum now acts, after its effusion, and owing to the peculiarity of the conditions of the cerebral circulation, by pressing on the outer wall of the vessels, by diminishing their lumen, and thereby causing an anæmia of the brain, which anæmia is assumed to be the cause of the convulsive phenomena. That, as I take it, is the Traube-Rosenstein theory, and you will observe that the physiological phenomena here described cannot but be regarded with deep interest, and may well be admitted to have an important bearing on the subject which we are considering.

There is only one other theory to which I would call attention, differing in some respects from the one which I have just mentioned, and which we owe to Dr. Angus M'Donald of Edinburgh. Dr. Angus M'Donald carefully examined certain cases, in which he had an opportunity of making a *post mortem* examination, after death from puerperal convulsions associated with albuminuria. In the course of these investigations he was assisted by Dr. Hamilton, when it was found that, in so far as the brain anæmia was concerned, the condition of the brain, described by Traube and Rosenstein, was confirmed. They failed, however, to discern any evidence of œdema of the brain, and were thus forced to look for some other theory by which to account for these epileptic seizures; and, by a process of very close reasoning, conducted in the most careful manner, as any one will see who takes the trouble of reading Dr. M'Donald's paper on the subject, he comes to the conclusion that the probable cause of these convulsive phenomena is a poisonous action of some kind or other upon the vasomotor centres in the medulla oblongata, the result being a spasm in the capillaries of the brain, causing the anæmia, and giving rise to the convulsions. That, so far as I know, is the latest theory bearing on the subject. If we go back to the question of albuminuria occurring during pregnancy, and if we take the phenomena that offer themselves to our consideration in any given case, our attention is first awakened by the fact of their being œdema of the ankles. We then test the urine; we find albuminuria; and we are naturally apprehensive of the results which sooner or later may accrue. It may be that puerperal convulsions are set up, and we not unnaturally do what so many others have done before; we look upon the phenomenon first observed as the cause of those which follow later on, and we, not unnaturally, think that the convulsions have been caused by the albuminuria. Now, are we quite sure that, in coming to this conclusion, we are on the proper line, and are we quite sure that our mode of reasoning is correct? I am not at all certain that we are entitled to answer that question confidently in the affirmative. On the contrary, I think it is quite within the bounds of possibility that the cause of the albuminuria and the cause of puerperal convulsions alike may be a toxic cause, when the case may fall under the category of which Dr. Finlayson has spoken. You have certainly a deteriorated and impoverished blood circulating within the vessels. If, in addition, you have a toxic agent—be it carbonate of ammonia or anything else—may it not be conducted to the vasomotor centres, and, by inducing contraction of the capillaries, cause œdema of the brain? If the cerebrum be mainly involved, coma will be a prominent symptom; whereas, if the motor centres are chiefly affected, then the severity of the convulsions will probably be in direct proportion, and will be more or less strongly marked. That is surely a possibility. And then, again, the same toxic cause acting as Dr. Finlayson would seem to indicate, may be the cause of the albuminuria, and that, mark you, would account for what we often observe, and what, as far as it goes, is against the idea of the albuminuria being the cause of convulsions, that we have often albuminuria existing to a marked extent without convulsions, and we have other cases in which there is no albuminuria and in which we have marked convulsions. I merely throw out that idea as a question that may be considered along with others which fall to be looked to in connection with this important subject.

I would only add one word more before sitting down, to the effect that I trust that, before this discussion is closed, we may hear something more of the clinical and therapeutic aspects of the case. It is often said, with regard to purely pathological research, that it is astonishing how seldom it points to anything practical. It is not altogether a reproach, because that, strictly speaking, is not the immediate object of pathological research, but I would call attention to the fact that this is a Clinical as well as a Pathological Society, and I am not without hopes that, before the discussion ends, those busy practitioners amongst us who have not too much time to devote to purely pathological speculation, may carry away with them some idea of what the best and most reliable treatment is in albuminuria in its broadest aspects. And, though we may not attain, as the immediate result of this discussion, anything very new with regard to treatment, I do not doubt that what has already fallen from previous speakers has contributed a great deal to the elucidation of the subject, and that the atmosphere has been cleared of a great deal that was obscuring our vision. I cannot but hope—I confidently believe—that this discussion will be a stepping stone to more correct pathological ideas, and more satisfactory therapeutical appliances, when the whole question shall come to be more thoroughly understood.

The Pathology of Albuminuria.

By PROFESSOR GREENFIELD, M.D.

After the very elaborate papers which have been read it is unnecessary for me to speak upon some of the subjects to which I intended to refer. The able and ingenious paper in which Dr. Newman opened the discussion raised many questions which have not been so fully considered as they deserve, and I shall endeavour to discuss some of them. With the admirable remarks of Professor Gairdner on the clinical and practical aspects, and those of Dr. Finlayson on the causation of albuminuria, I agree to a very large extent.

It appears to me that it is of especial importance in the study of the causes and clinical significance of albuminuria that we should know what are the exact changes which are present in the kidneys in those forms in which there is any discoverable lesion: and it is especially to this question that I shall direct my remarks. But I may first say a word or two with regard to some other points which have been raised.

And I may first remark that if by albuminuria we mean the presence of serum albumen in the urine in easily recognisable quantity, I do not think we are justified in speaking of a *physiological* albuminuria. By a physiological albuminuria we should mean a normal albuminuria. As the term has been used, "physiological" has been made to cover all those forms of albuminuria in which no traceable organic disease is present. Now, by a pathological albuminuria I mean an albuminuria produced by derangement of function, as well as by derangement of structure. If albumen can appear in the urine as a result of vasomotor paralysis, dependent on central nerve disease—and under disease we must include derangement of function—it is none the less a pathological albuminuria, however slight may be the derangement of health which produces it, and however it may act. It is an entire misuse of terms to speak of this as physiological albuminuria. The asserted presence of albumen in the urine of healthy persons is one of extreme importance. The statements made by reliable observers support the view that albumen may appear now and again in the urine of apparently healthy persons. Such observations as those of Leube and of Dr. Mahomed may be regarded as authoritative. With regard to Dr. Saundby's statistics, they are so entirely contrary to the experience of others that I am convinced there must have been some fallacy in the method of observation. It is a remarkable thing that it is extremely rare to find albumen in the urine of candidates for life assurance who are otherwise apparently healthy, and that with the systematic examination of the urine in hospital wards cases rarely occur in which we cannot assign some definite cause for its presence. There are cases—I have had such under my own care—in which there was no evidence of disease beyond the presence of albumen in the urine, and in these cases not merely intermittently, but in one or two cases continuously, during a period of some months, though in variable quantity. In the cases to which I refer there was neither dropsy nor cardio-vascular change, and none of the various disturbances which we find in Bright's disease; the subjects were especially anæmic young women. The urine was usually pale, clear, of low specific gravity, often phosphatic, and contained neither casts nor blood. But are we on this account to say that there is no disease, that it is a physiological albuminuria? I believe that as pathologists we are bound to allow of no such conclusion without the most exhaustive analysis and conclusive evidence. Structural alteration we may be unable to detect, but the greatest advances in pathological research have been made in the endeavours to discover organic lesion where none was apparent, and it would be a retrograde step if we as pathologists allowed that so important a condition could exist without definite organic cause.

There is one fallacy in particular that I may point out. It is a well known fact that extensive disease may exist in the kidneys, remaining for a long period entirely latent, and reveal itself by some terminal fatal phenomena, or be discovered only after death. I have for some years made it a special object to examine all kidneys, whether obviously diseased or no, whenever opportunity permitted, more especially in cases of men dying over the age of 30, or dying from acute diseases of all kinds, and have been struck with the frequency with which changes of an extensive character have been found where none were suspected during life. In the case of accidents or operations which do badly, and in fatal pneumonia and erysipelas one frequently finds extensive renal changes; and even where there is no obvious naked eye lesion we constantly find in persons, especially men of the ordinary intemperate habits of the working classes, extensive degenerative changes belonging to the same kind as those found in chronic Bright's disease.

Now, this being the case, the presence of albumen in the urine from time to time may be, and I am convinced frequently is, an indication of acute renal change, limited perhaps for the time to a small area of the renal tissue.

Still further, the defective recognition of the morbid appearances in diseased kidneys frequently leads to errors even on *post mortem* examination, and I have repeatedly detected in kidneys which had been sent to me as perfectly healthy, evidence of advanced disease.

Such being the case, and the well known facts of the latency and chronicity of Bright's disease being kept in view, we, as pathologists, cannot admit a physiological—that is to say, a healthy albuminuria.

There is another point of a clinical nature to which I would for a moment allude, namely, the curability of Bright's disease, both in its more severe and in its slighter forms. The kidney has been looked upon as if it were an organ outside the control of ordinary physiological and pathological laws. We speak of diseases of the kidney as if they were some mysterious phenomena differing in their course and relations from the diseases of other organs. Now, this is not only pathologically but clinically an error. The kidney, as Dr. Gairdner has well remarked, is liable to a large number of diseases precisely similar to those of other organs, and there is no conceivable reason why disease of the kidneys should not be curable just as are diseases of other organs. Scarlatinal nephritis, for example, however severe, may, as we know, be perfectly cured so far as the healthy performance of function, and what we call practically health, is concerned. We must all have seen cases of persons who had been at the point of death from post-scarlatinal dropsy, with scanty highly albuminous urine, who are now after the lapse of years in good health, with functionally healthy kidneys, and without cardio-vascular changes; and, as I shall endeavour to show, post-scarlatinal nephritis is closely analogous to other inflammations of the kidney in its later stages. I hope later to refer to a case of cure of acute nephritis where I had an opportunity of examining the kidneys after death from other disease. And I have seen some of the lesions of more advanced Bright's disease completely disappear under treatment. I had under my care for more than two years a man who was suffering from what is called chronic inflammatory Bright's disease, which had come on with an acute attack more than a year before. He had marked dropsy, considerable hypertrophy of the heart, and high arterial tension, scanty urine always containing from $\frac{1}{3}$ to $\frac{1}{2}$ albumen; he was able, notwithstanding, to continue a light occupation. He developed under observation most marked albuminuric retinitis, which was of the degenerative form, with patches around the macula lutea, and served for some time as a subject for demonstration of that disease. Under observation, which was confirmed by my friend Mr. Nettleship, the retinitis entirely disappeared, the patient being treated by the old fashioned methods of free saline purgation, iron, and a largely milk diet.

Such facts as these, and the important observations in scarlatinal albuminuria quoted by Dr. Mahomed, show conclusively that many of the forms of Bright's disease are capable of great amelioration or of absolute cure, but just like an old pleurisy or an old pericarditis they usually leave behind structural changes which, though compatible with health, are readily recognised by the morbid anatomist.

Before I refer to the pathological changes, I may briefly mention one or two points in reference to the normal anatomy and physiology on which I must differ from Dr. Newman.

He has especially referred to the relative size of the afferent and efferent vessels of the glomeruli as an important factor in their circulation. But it must be remembered that although there is usually one efferent vessel, the exceptions are not rare in which we find more than one.

Again, there is this difference, that the afferent vessel is highly muscular, the efferent mainly elastic. The afferent is probably normally in a state of tonus, and readily contracts or dilates under the influence of stimuli. So that the relative calibre during life cannot be judged from the results of injections.

In the normal condition of circulation the volume of blood entering the glomerulus must equal that issuing, and the velocity will be, speaking roughly, inversely to the area of section. So that to speak of the circulation in the glomerulus as normally under increased pressure owing to the relative calibre of the vessels is an assumption not warranted by the facts. In other words, it has not been proved that the transudation of water in the quiescent condition requires any greater pressure than does the normal transudation of lymph from the capillaries.

If, however, the arteries be relaxed, the velocity of the stream issuing through the efferent vessel will be increased, and at the same time the pressure will be raised in the glomerulus, and if this exceeds that in the tubules increased transudation will occur.

As regards the other conditions of filtration of water from the glomerulus, we know that the condition of the blood, especially with respect to certain constituents, increases or diminishes it. Thus some saline bodies cause increased transudation, but whether they act directly upon the membrane or on the vessels, or their local vasomotor mechanism, we do not know. That this action continues when the nerves are divided we do know.

And let me add, in case I have no time later to speak of the results of the granular contracted and the waxy kidney, that the polyuria, which occurs during the former and the early stages of the latter, may very probably be due to loss of vasomotor control.

In one of the forms of cirrhotic kidney, as I hope to show, the primary or earliest change is a fibroid change in the arterial walls, which especially affects the afferent arterioles, and whilst it reduces their calibre greatly impedes their contractile power. So, too, in the earliest stage of the waxy kidney, the middle coat of the afferent arteriole is usually the first part to be diseased. So that in place of the normal alternations of pressure, and the regular diurnal passage of urine with long intermission at night, there is a continuous slight increase in the pressure, and hence a constant trickling of water through the tubules, which gives rise to the frequent micturition and its continuance during the night. I do not say that this is the *sole* cause, but it is probably an important one.

In cases where there is simply arterial degeneration without other structural change in the kidney, this polyuria may be accompanied by great increase in the excretion of urea; thus, in a case of the kind in which I examined the kidneys recently, the quantity of urea reached 800 grains per diem.

Now, let me pass for a moment to the question, *Does albumen normally transude into the renal tubules?* If we find that a large number of altered conditions, many of them of extremely slight character, cause the presence of albumen in the urine, is it not in favour of the view that there is a mechanism, whatever may be its nature, which is very readily deranged, but which normally prevents the passage of albumen. If fevers of all sorts, and blood poisonings, various vegetable and mineral poisons, derangements of circulation due to central nerve lesion, alterations of pressure which we may call mechanical, produce albuminuria, must there not be some easily disturbed mechanism by which the passage is ordinarily impeded? Is there any great difficulty in the view that water only, or water with some readily diffusible substances alone passes out of the glomerulus?

If we compare the transudations from serous surfaces with each other, they contain as we know very variable proportions of proteids, including serum albumen. But the glomerular tuft, with its epithelial covering, is not the analogue of a serous membrane: may we not rather compare it with the salivary and gastric glands, from which very large transudations of fluid containing an extremely small quantity of proteids, and those of special kinds, and no serum albumen, is constantly secreted. For the capillaries are covered by a layer of epithelium, and although it is but a delicate thin layer, there is no ground for denying its secretory function.

The epithelium of the pulmonary alveoli is a still less distinct layer, yet it appears to suffice to prevent the proteids from transuding under normal conditions. The glomerular capillaries no doubt allow the transudation of the proteids of the blood, but so long as the epithelial covering is healthy there is no reason why these should pass into the tubules.

If time permits, I may bring forward some pathological evidence in favour of the view that the epithelium covering the glomerulus reacts in the same manner as a mucous epithelium, whilst that which lines the capsule reacts in the same way as a serous endothelium under pathological stimuli.

It must be allowed that the theory which has been mentioned as Ludwig's, but which I believe Ludwig has now abandoned, is a very seductive one, but I do not think it is now maintained by any physiologist who has made a special study of the renal secretion, and the weight of clinical, as well as of physiological and pathological evidence is entirely against it. The problem has not yet reached the stage of final solution, it requires further experiment, and I do not propose further to discuss it. But as illustrations of the practical difficulty in accepting this theory, we may ask, If albumen normally transudes from the glomeruli, why is it that in cases of hysterical or other nervous polyuria, in which an enormous quantity of urine is passed under the influence of a definite emotional excitement, and within a very short time, albumen is not constantly present? And why, in diabetes, where the quantity of water may reach 300 or 400 ounces per diem, does the presence of albumen usually indicate degeneration of renal structure, or some secondary complication? Again, why in many cases of advanced cirrhosis of the kidney, where the quantity of urine secreted is often large, and where the epithelium and the walls of the tubules have undergone very serious structural alterations, do we so rarely find albumen present?

I may now pass to the question, What is the intimate pathology of albuminuria? I must, owing to want of time, omit all mention of the conditions present in fevers, and in other states where there is no recognisable organic lesion in the kidneys, and consider only the conditions which we find in those forms of renal disease in which albumen is present. We may then inquire, How do they explain the presence of albuminuria?

Now, any satisfactory answer to this question must take into consideration all the varied conditions which may produce it, and we must not limit our attention to those which support or contradict one particular view; and in order to state any of the facts clearly it will, I fear, be necessary for me to enter with some detail into the minute changes found in the kidneys in the various forms of Bright's disease. For it is impossible, without minutely studying these changes, to form any just opinion of their effects; and I shall ask your permission to leave for the moment theoretical deductions whilst I state some points which do not appear to be sufficiently appreciated in regard to the morbid anatomy of the kidney, especially in those forms of disease which are related to albuminuria. We may group the more important of these diseases under waxy degeneration and its associated changes, inflammatory processes in the kidney (acute and chronic), and chronic degenerative changes secondary to vascular degeneration. We must also take into consideration the effects of chronic venous congestion.

Various references have been made in the course of this discussion to the occurrence of albuminuria in heart disease, as evidence that increase of venous pressure produced transudation of albumen. The statements and arguments employed are in favour of the view that so long as there is marked venous obstruction, albuminuria should continue.

Now, as we know, in chronic bronchitis, in chronic mitral valvular disease, &c., there is constant obstruction to the venous reflux, and as a result we have that enormous distension of the venules of the kidney, including the interlobular veins, the vasa recta, and the intertubular and glomerular capillaries which we find in such cases. We have, too, as a secondary result, the thickening of the capillary and venous walls which corresponds to that seen in the lungs in chronic pulmonary vein obstruction.

But if we compare these pathological facts with the clinical in any case we find that albuminuria is usually *not* a constant occurrence; that albumen commonly occurs in the urine paroxysmally coincidently with scantiness of the urine, and usually with marked aggravation of dropsy and cyanosis—that we very frequently find also some blood in the urine. And if we find albumen constantly present where there is heart disease, we may be led to suspect that there is other independent renal disease. If there is arterial tension in spite of mitral incompetence or stenosis, we may correctly diagnose chronic nephritis; if only many cell casts and hyaline casts it may be merely catarrh, especially of the straight tubes, the congested kidney, like the congested stomach or bronchi, being more liable to catarrh.

Now, if we exclude the cases in which the presence of albumen and blood in the urine is due to embolism, how are we to explain these phenomena? Either the venous obstruction is not constantly attended by increased pressure in the glomeruli, or such increase of pressure is not alone a sufficient cause of increased transudation either of water or of albumen.

But we know that the condition of the glomerular capillaries after death indicates that they have been subjected to considerable pressure, for they are dilated and their walls thickened, as elsewhere where venous obstruction has existed during life. And we might conclude from this that whereas increased pressure due to arterial dilatation promoted the watery flow, increased pressure due to venous obstruction retarded it, or that venous blood was less favourable for the transudation, or that the thickened capillary walls opposed an obstacle to filtration. But there are some other conditions which must be considered before we can arrive at this conclusion.

The venous obstruction is first felt in the vasa recta and the intertubular plexus. The distension of the vasa recta is so great that it appears to cause pressure upon the straight and looped tubules in the pyramids. This must tend to raise the pressure in the tubules and in the glomerular capsule, and as this more nearly approaches the pressure in the glomerular capillaries, the transudation will become less abundant. Moreover, the distended intertubular plexus will keep up pressure on the surface of the capsule and will prevent a disproportionate dilatation of the glomerulus. Apart from this, the greatly lowered arterial pressure will, as we know from physiological experiment, tend to diminish the amount of secretion.

But, owing to the shortness and directness of the afferent arteries, the pressure is not lowered to a sufficient degree entirely to arrest, though it suffices to diminish the excretion.

Notwithstanding what has been said by Dr. Newman, I think it probable that the velocity of the blood current through the glomerulus is considerable, compared with that in other capillaries. For although it is true that the efferent vessel is smaller than the afferent when the two are fully dilated, the afferent is capable of complete muscular contraction, whilst the efferent is not, but dilates to its maximum under internal pressure.

The glomerular capillaries are relatively short, and when once the circulation is established through them there is no reason why there should not be a constant circulation with very little change of pressure. Probably, as I have said, there is but little *constant* transudation through the glomerulus, this occurs when the pressure is raised by dilatation of the arteriole.

On the other hand, the circulation through the intertubular plexus is apparently slow, the blood being under a low pressure. This appears to be a condition favourable to the elaboration and excretion of waste products, and is seen also in the liver. In the amphibia, as we know, the urinary salts are excreted from venous blood. The stagnant circulation would not, therefore, seem to be unfavourable to the excretion of waste products, although they may be defectively oxidized.

A sudden increase of pressure may cause transudation of albumen, and usually in chronic venous obstruction this is so great that some of the capillaries rupture and blood also passes into the tubules. How is this brought about? Probably in one of two ways. It is frequently coincident with increased embarrassment of circulation. But this is usually dependent on or associated with cyanosis. Now, in carbonic acid poisoning the arterioles of the body generally are in a state of spasm, hence the high arterial tension sometimes seen in grave bronchitis. But it would appear that the renal arterioles do not react in the same way or not to the same extent. The effect of such general increase of arterial tension will be proportionately to raise the pressure in the renal arteries, and thus cause a twofold pressure in the glomerulus, backward and forward.

Or, the same result may be brought about where there is ascites. The additional pressure on the inferior cava, or on one or both renal veins, may cause sudden increase of pressure in the glomeruli. I have not gone into the question of degenerated structure. That this is not *essential* seems to be shown by occurrence of albuminuria as a result of partial asphyxia—*e.g.*, after fits, immersion, &c.: and in these cases we may also find hyaline casts in the urine; but it is of course possible that it may be an accessory, especially where the albuminuria is constant.

We may, then, in spite of the intermittent character of the albuminuria in chronic venous obstruction, regard it as a case of its production by increased pressure in the glomeruli. It is, indeed, possible that some direct passage of blood may take place from the engorged vasa recta into the tubules.

The cessation of the albuminuria and the promotion of diuresis may, as we know clinically, be brought about by diminishing the venous pressure and the cyanosis, or by increasing the arterial pressure. And the pathological explanation is found in the facts which I have mentioned.

I must now pass to those forms of renal disease, of which albuminuria forms a common symptom, and I shall especially refer to the various forms of nephritis, acute and chronic. Waxy disease and its various complications I must omit from consideration, owing to want of time.

Now a distinction has been drawn and sharply maintained between what is called catarrhal or tubular inflammation and interstitial inflammation of the kidney. I have long maintained that, although catarrh plays an important part in various forms of renal disease, this absolute distinction is one which cannot be supported by pathological or clinical evidence.

Let us put the problem to any one who has studied the pathological changes in inflammation of other organs, and who is acquainted with the structure of the kidney, but not with its pathological changes, as to what condition will be found if the kidney is subjected to inflammation.

Let us take first the vascular phenomena of inflammation; and the answer will be—the effect of inflammation in the kidney, however produced, will be as in other organs, primarily a vascular change. There will be a stage of active congestion, followed by the phenomena of slowing or stasis, accompanied both during congestion and stasis, by an increased transudation of lymph and diapedesis of leucocytes. The points at which these changes will be most marked will be around the small venules, and to a less degree the smaller arteries.

Apply this to the kidney; the points at which this transudation would occur in its greatest degree will be around the efferent vessels of the glomeruli, which are the first venules, and around the smaller arteries. A similar effect will be produced along the vasa recta, and we shall also find the lymph-spaces distended with more highly albuminous fluid, and exudation of leucocytes in the tissues around. The effect of this upon the other structures, if the inflammation is not intense or prolonged, will be at first impaired nutrition of the glandular cells, and very probably some active proliferation.

Now this is precisely what actually occurs under a large number of conditions; in the slighter forms of acute so-called renal catarrh, in the forms produced by poisons of various kinds, and in the more severe forms associated with the poison of scarlet and other fevers. We find in these cases that surrounding the vessels at their passage through Bowman's capsule there is a great increase of cells, mostly leucocytes, and distension of the lymph spaces. If the irritation is sufficiently intense or prolonged inflammatory change may extend widely along the inter-tubular plexus.

Now let us consider for a moment what will be the effect upon so delicate a structure as the glomerulus. The exit from its vessels is impeded by the pressure around the efferent vessel, which is far more affected by such pressure than the muscular afferent arteriole, and there will of necessity occur mechanical turgescence of the glomerular capillaries, and a condition which may of itself suffice to cause the transudation of blood serum. If very intense, as in some cases of poisoning—I might instance a well marked case of corrosive sublimate poisoning, and other cases which have come under my observation—not only serum-albumen will transude, but a large quantity of fibrinous substance, and frequently also blood. The condition of the glomerulus may be likened in its mechanical causes to the choked disc, produced by distension of the sheath of the optic nerve.

I have taken a simple illustration of the effects of acute inflammation upon the individual glomerulus; but I may presently show that precisely similar conditions are present in a number of less acute inflammatory processes in the kidney. To keep for the moment to the glomerulus. If the irritant be sufficiently intense, or the inflammation be sufficiently prolonged, there will ensue, as in other organs, proliferative changes, whether the proliferation is of leucocytes or of connective tissue corpuscles—a much debated question —we need not for the moment consider. That there occurs around the afferent and efferent vessels a growth of connective tissue, at first cellular, and later becoming fibrous, which extends around the Malpighian capsule, and for a certain distance into the intertubular plexus. The vascular outlet being thus obstructed, a secondary reaction occurs in the capillaries of the tuft by which they proliferate and become occluded. The arteriole becomes closed by a thrombus, and endothelial proliferation rapidly obliterates it. A secondary irritation of the epithelium lining the capsule and covering the tuft causes adhesion of the two opposed surfaces, and by gradual steps the Malpighian body is transformed into a fibrous nodule continuous with the surrounding connective tissue.

I have assumed that the inflammatory changes are limited to the region of the afferent and efferent vessels, but this is not strictly speaking the case, for the inflammation affects also the Malpighian body—in intense acute inflammation at an early stage, in chronic inflammation at a later period— and we shall see in consequence proliferative changes which lead to gradual obliteration of the glomerulus, which are indicated partly by a multiplication of the cells in the substance of the tuft, and more especially by proliferation of, and organisation of connective tissue from the capsular epithelium.

Now, in a somewhat modified form this description might be applied to all the cases of acute and sub-acute inflammatory nephritis, and to the chronic changes, partly degenerative, partly of the nature of proliferative degenerations, which occur in the various forms of the cirrhotic kidney, although there are also other important changes.

In the same way, and at the same time as in the glomeruli, acute inflammatory exudation and diapedesis take place around the vasa recta, the lymph spaces become distended with a fluid more highly charged with proteids, and leucocytes in variable number transude from the walls. Around the larger vascular branches also, including the interlobular vessels, there occurs similar exudation, and where the inflammation is sufficiently intense, or is chronic, proliferative interstitial changes occur. Before I consider how these specially vascular changes affect the presence of albumen, let me say a word as to the condition of the tubules. Apart from any primary change in the gland epithelium, such as may be induced by various irritants brought into contact with the epithelium, either through the blood-vessels or the urinary secretion, as in some forms of poisoning, the nutrition of the epithelium must of necessity suffer from the changes which take place in its vascular supply.

The obstruction to the intertubular circulation—the derangement of the intertubular circulation produced by glomerular inflammation—damages temporarily or permanently its nutrition. But it is a curious fact, which may be especially observed in post-scarlatinal nephritis, that very considerable derangement is not necessarily attended by any marked catarrhal or destructive change ; cloudy swelling and fatty degeneration are frequent, but they may not reach any high degree. If, however, the derangement becomes more chronic, the nutrition of this epithelium, especially of those parts in most immediate contiguity with the damaged glomeruli, suffers seriously, and the epithelium undergoes degenerative changes, especially fatty degeneration, atrophies, and is to a considerable extent absorbed. If the inflammation is intense, or if the irritant exciting the inflammation, as in the case of some poisons such as those in malignant jaundice, and some metallic poisonings, directly damages the gland epithelium, acute necrotic processes or intense catarrh may be produced. On the other hand, in those tubules which are of the nature of gland ducts we find early and intense catarrh, especially in the straight tubes in the immediate vicinity of the vasa recta, and also in the looped tubes in the same position, the channel of the tubules in many cases being completely obstructed by a mass of cells which closely resemble pus corpuscles.

This change is found very early in all acute inflammations of the kidney, however excited, and is apparently the cause of the so-called epithelial casts, composed of masses of small cells, which are so abundantly found in the urine in acute nephritis.

Whether these cells are derived, as appears possible, from an exudation of leucocytes directly from the blood-vessels into the tubules, or from proliferation of the .epithelium, is a point I have found extremely difficult to determine. In some of the more intense inflammations it has appeared to me that they most closely resemble degenerated leucocytes, in other words, pus corpuscles.

I had under my care a short time ago, a patient who passed urine containing a large deposit of pus. This pus-like layer had microscopically all the characters of ordinary pus, with the exception that here and there the pus cells were arranged in short columnar masses, but not with sufficient regularity to enable one to call them casts.

This patient died, and at the *post mortem* the kidneys were found to be enlarged and mottled; microscopically, they showed the characters of a somewhat acute so-called catarrhal nephritis, and all the straight tubes, and many of the looped tubes, were filled with these pus-like cells.

If, however, we must accept the catarrhal view of the nature of nephritis, the presence of marked interstitial inflammation, associated with these catarrhal changes, is precisely analogous to what we see in chronic catarrhs of mucous membranes, and it is unnecessary to attribute to the epithelium—especially so highly organised an epithelium as that of the convoluted tubules—any special proneness to catarrh.

Let us now consider the immediate cause of albuminuria in the acute forms of Bright's disease. When we consider the variety and the nature of the lesions which I have described, we see that we have various grave lesions, any of which might readily produce albuminuria. Transudation of blood-serum from the glomerulus may at first be the result of increased pressure; later, of the outflow of inflammatory lymph. The epithelium covering the glomerulus may not only be abnormally stretched, but its cement substance may be loosened, and its cells undergo inflammatory changes, so that until repair occurs, or until, by adhesion of the apposed surfaces of tuft and capsule the transudation is impeded, albumen must almost of necessity escape into the tubules. In the same way transudation of albumen, and even of leucocytes, may occur directly from the vasa recta, or from the exudation from the vasa recta, through the thin walls of the tubules, especially if these are damaged by catarrh. Possibly, also, although I believe to a far less extent, a similar transudation may occur from the intertubular plexus into the convoluted tubes; but were this the case we should probably find more intense changes in this epithelium than usually are present, and it appears more reasonable to suggest a transudation into the straight tubes, or descending looped tubes.

There can, therefore, be no difficulty in accounting for the presence of albumen in the urine, even in the slighter forms of acute Bright's disease, and its sequelæ; the difficulty is to select its source. In connection with this I may advert to a point suggested by Dr. Newman in relation to a case of acute parenchymatous nephritis. His theories of the re-absorption of water and the non-absorption of albumen in this condition were grounded upon a case in which the patient died during the acute stage of the disease. But why did he not go further, and take a case at a later period, when the urine continuing scanty and highly albuminous, the anatomical conditions are so entirely altered that his explanations could no longer serve to account for the state of the urine?

Let me give some account of these conditions which are found in the form of renal disease following an acute inflammatory attack. This form is one of the most common and the most difficult to treat, and hence it is of especial importance to determine its exact nature. It is frequently assumed that it is a purely catarrhal disease, and that it differs entirely in its pathology from chronic interstitial nephritis. This I hold to be an error due to incomplete investigation, and whilst there are certain distinctions between cases which have an acute onset, and those which are chronic in their entire course, it is not possible to draw an absolute line between them.

The pathological process is essentially the same, whether the disease have its origin in an acute attack, scarlatinal or other, or is associated with pregnancy, or due to any other definite cause, or if it commences insidiously with recurrent slight attacks.

Let me return to the point at which I left the subject of acute inflammation. I have said that in intense inflammation of the kidney there frequently occurs very extensive diapedesis and interstitial exudation of lymph, just as in other acute inflammations, so that we find that these changes exist to a variable degree—that they may be limited to the region of those vessels around which the inflammation begins, or may extend widely along the other vessels. In rare cases the capillaries are separated from the tubules by a quantity of exuded lymph; this is especially in very acute inflammation. What are the further changes which take place after this acute inflammatory change? The inflammation may undergo cure; the epithelium may, it is true, be partly detached in the tubules, but if the inflammation has not been very intense it may entirely recover and return to its normal condition, or what has been detached may pass out as casts, or be partly absorbed by the lymphatics. When this cure takes place, and we examine the condition after the cure has become complete, we usually find that there are certain patches in the kidney in which there has been this inflammatory exudation, and in which there has been formed in place of it a certain quantity of fibrous tissue, the tubules having undergone atrophy.

But together with this we always find extensive changes in the Malpighian bodies. By the changes which I have described, the glomeruli are either entirely obliterated or the capillary tuft becomes partially or wholly adherent to the capsule, and a consequent fibrous growth and atrophy occur. It is probably this change which causes such serious permanent damage to the kidney, where inflammation is intense or prolonged. If limited to only a certain number of glomeruli, or to certain regions of its substance, the corresponding tubule may atrophy, but the remainder of the kidney suffices to carry on the function.

But in many cases the inflammation does not altogether subside; it becomes chronic, and is readily re-excited. Exposure to cold, constipation, and even indiscretions in diet may serve to renew it. We need not stop to inquire now how these causes act. Probably it is both by increasing the congestion, and by giving the kidney more work to do. In these cases, where death may occur at very various periods after the attack, it is very exceptional to find merely the fatty degeneration of desquamated epithelium, which was formerly described as characteristic. Out of a very large number of specimens of diseased kidneys, I have only two which could be said to show this condition as the most prominent. And one of these was a case of very severe nephritis in a child, almost certainly post-scarlatinal, fatal in about six weeks; the other, a case of a patient with pneumonia, in which there had been no marked renal symptoms until two days before death.

As a rule, we find that if the inflammation becomes chronic it leads to the condition which I have described as sub-acute interstitial nephritis. The naked eye appearances of the kidney vary according to the stage and extent of the disease, the amount of fatty degeneration, and the degree of congestion. It differs from the granular contracted kidney in its size, the smoothness of the surface, uniform thickening of the cortex, and absence of cysts. In some marked cases the kidney has been described by competent pathologists as pale, but otherwise perfectly healthy.

Microscopically, under a low power, the most marked changes are seen in the cortex. The glomeruli are fairly normal in position and arrangement, but appear more homogeneous than natural, and are surrounded by a close texture in place of the open network of the tubules. In the region of the medullary rays we find the tubules apparently dilated. Thus there are somewhat pyramidal bands passing inwards from the surface, corresponding with the region of the glomeruli, arteries, and convoluted tubules, in marked contrast with the region of the medullary rays.

If we study a little more minutely the condition of the Malpighian bodies we find that nearly all of them show a laminated thickening of Bowman's capsule, this laminated thickening being continuous in the interior with the layers of flattened cells which lie upon it. Or the capsule may be simply thickened and laminated, not showing any distinction from the tissues around, and continuous with the capillary walls, and adherent to the tuft. A certain number of the Malpighian bodies have undergone still further processes of change; they have become transformed into concentric fibrous knots in which we can see no capillary structure, no distinction between the wall and the capillary tuft, the capillaries almost entirely atrophied. This thickening of the capsule is continuous outside with an enormous overgrowth of connective tissue which lies between the tubules. The tubules are separated, and their epithelium is degenerated, or fatty, or has become flattened, and has undergone such changes that we can no longer distinguish between the various parts of the tubules, and we cannot say which has been collecting and which convoluted tube—owing to the extensive changes in the epithelium. In addition to these glomerular and interstitial changes there are very marked changes in the vessels, including both the large trunks and the interlobular arteries. They have undergone a very marked degree of thickening, partly of the intima, which has undergone great swelling, so that it may nearly close up the vessel. The middle coat becomes apparently thickened but probably really atrophies—undergoing a sort of fibrous thickening—and the outer coat also becomes enormously thickened; so that these vessels look on section like thick fibrous rings, and some may be almost indistinguishable from an atrophied Malpighian tuft. These changes are not limited to any set of vessels, but they affect the large vessels, and those even to the smallest size.

But from the fact that some quite healthy arteries may usually be seen, it is probable that this thickening is partly due to chronic inflammation, and is such as we see elsewhere in granulation tissue.

In some cases we find similar changes around the vasa recta.

It is in cases of this nature that albuminuria persists for months or years, and usually also a varying degree of dropsy, in which also arterial tension becomes very marked, and hypertrophy of the heart occurs early and steadily progresses. It is not difficult to account for the occurrence of albuminuria in such cases. The extensive damage to the Malpighian bodies would alone suffice to explain it. But we have in addition the damage to the walls of the tubules and the deranged circulation in the kidney. The remarkable thing is that an intermission of the albuminuria sometimes occurs, and the urine becomes at the same time greatly increased in quantity.

No absolute line can be drawn between this form of renal disease and that known as renal cirrhosis or granular contracted kidney. Indeed, it is an open question whether, in a considerable number of cases, the latter has not its starting point in an acute inflammation.

But if we carefully inquire into the history and the renal changes we find that at least two forms of disease are grouped under this common name of renal cirrhosis, the one essentially of a chronic inflammatory nature, the other due to an atrophic process dependent on a primary arterial degeneration. It is true that we find these two forms intermingled in some cases, but in others they appear absolutely distinct.

In the chronic inflammatory form, the changes found on microscopic examination are in their essential nature identical with those in the sub-acute interstitial nephritis, but differ in the fact that the process usually commences in limited parts, especially in the glomeruli nearest to the surface of the cortex, though sometimes still more marked in those which lie deepest near the large vessels, and that the evolution is more chronic. We find the same glomerular changes, the same intertubular connective tissue of variable amount, the same atrophic and degenerative changes in the tubules. But the arterial change is more widespread; the capsule is much thickened and adherent, and irregular tubule dilatation and cyst formation are present.

In this form of kidney disease then the glomerular changes are always more extensive than those in the intertubular tissue.

The clinical conditions met with vary. The urine is sometimes highly albuminous during long periods; but in most cases the albuminuria is intermittent, though frequently occurring in small quantity.

In the chronic atrophic form the primary change is a chronic peri- and endo-arterial fibrous thickening. Owing to the essential continuity of the peri-arterial fibrous tissue with Bowman's capsule, the latter frequently shows marked fibrous thickening at an early stage. This, together with the obstruction produced by the thickening of the arteries, produces gradual occlusion of the afferent arteriole, and the glomerular capillaries undergo a fibrous atrophy. This change occurs to the greatest extent at the periphery of the arterial system, hence it is most marked towards the surface of the cortex. As the glomeruli become obstructed and atrophy, consequent atrophic changes occur in the tubules, and the cortex may thus be entirely transformed into a thin shell, consisting of atrophied glomeruli and wasted tubules. Strictly speaking, this is not an interstitial nephritis at all, although some intercurrent inflammation may occur.

In this form the disease may be almost entirely unattended by discoverable albuminuria throughout, and the chief evidence of its existence is the hypertrophy of the heart, and the high arterial tension—although there are frequently other general conditions to which I need not now refer. The urine is usually of low specific gravity and in large quantity.

I must not further discuss the pathology of the secondary changes, such as increased arterial tension and rigidity, and the occurrence of cardiac hypertrophy.

Nor must I refer, as I hoped to have done, to scarlatinal nephritis, which is one of the most valuable forms for investigation. I have already exceeded the due limits of such a communication. But I have desired to draw attention to these important changes, because no discussion of the pathology of albuminuria can be exact which does not proceed upon a due recognition of the exact morbid changes which are present in the kidneys.

On the Pathology of Albuminuria, with Special Reference to the Changes in the Circulatory Apparatus of the Kidney.

By JOSEPH COATS, M.D.

Mr. President and Gentlemen,—I desire to approach the subject of albuminuria from the pathological side. Perhaps I may be permitted to say at the outset that it seems to me that the subject has been hitherto treated too much from the merely physical or mechanical point of view. The kidney presents in its structure such intricate physical contrivances that, both in regard to the normal secretion of urine and the morbid alterations in that process, observers are prone to look merely to variations in blood pressure, nearness of the tubules to the blood-vessels and such like, in order to explain the phenomena. Most writers, I think, fail to apply the general principles of pathology to the case of the kidney, and regard rather the problem there as entirely a special one. This was particularly the case, as it appears to me, in the observations of my friends Dr. Newman, Dr. Mahomed, and Professor Hamilton. These gentlemen, I think, regard the kidney too much as a piece of dead mechanism, to which they can apply in their simplest form the physical laws which govern the mechanisms which human beings devise. And yet in the course of his remarks Professor Hamilton repudiated the comparison of the secreting or separating structure of the kidney with a piece of dead intestine, and his remarks in this regard were so admirable that I began to think that he was going to carry the same ideas into the domain of pathology. When he came to pathology, however, he seemed to forget that he had to do with a living structure and with the morbid alterations of that structure.

The problems before us are, How is it that albumen does not appear in the urine when in health, or at least does so in very few cases? and secondly, What are the causes of its appearance in the urine in disease?

In regard to the first of these, I do not at all agree with Dr. Newman's view of the case. The facts which he correctly stated in regard to the condition of the urine in acute Bright's disease on the one hand and chronic Bright's disease on the other, seem to me to contradict his view. In acute parenchymatous nephritis there is scanty urine loaded with albumen, and his explanation is that the albumen which normally passes through the vessels of the glomeruli is not reabsorbed, as the epithelium is paralysed by the inflammation. But the epithelium, though paralysed, is, it seems, quite capable of absorbing water so as to make the urine scanty. It seems strange to assume that the water is so largely absorbed and the albumen left. Then in the case of chronic interstitial nephritis we have polyuria and little albumen. This is just the converse of the other. Here the water is not absorbed; the urine enters the bladder very much as it passes from the glomeruli, and we are asked to believe that while the water is left alone the albumen is absorbed, for in many of these cases of extremely contracted kidney, the albumen in the urine is a mere trace. I shall return to this subject farther on, but I would here remark that in those cases of contracted kidney, the convoluted tubules of the cortex are greatly destroyed, the whole cortex is sometimes reduced to a thickness not much exceeding that of the finger-nail, and this implies great destruction of the convoluted tubules. Now, the convoluted tubules and their epithelium are undoubtedly the active elements of the system of tubules, and their destruction implies the removal of the most active constituents of this apparatus. Hence, I would say that the urine in this condition is nearly the normal urine, minus the influence of the convoluted tubules. It is nearly the urine as it passes through the glomeruli. This seems to me to confirm Bowman's and Ludwig's view that in the urine as it originally passes the glomeruli there is an excess of water, which is reabsorbed by the epithelium chiefly of the convoluted tubules. These tubules being atrophied, the water remains unabsorbed, and the urine as it passes into the bladder is very watery. In like manner, if albumen passed through the glomeruli, we should expect it also to escape absorption and to appear in large quantity in the urine, which it does not.

These are by no means the only arguments against the view which Dr. Newman has supported, but the question is too large a one to be fully discussed here, and I content myself with stating that I agree with the view that the normal urine as it passes through the vessels of the glomeruli does not contain albumen. The explanation of this remarkable fact has been already referred to by Professor Hamilton. The walls of blood-vessels everywhere exercise something of a selective power on the constituents of the blood which they allow to pass. Nowhere does the fluid transuded from the capillaries correspond in constitution with the blood plasma. The cerebro-spinal fluid, the peritoneal fluid, the synovia, &c., are, all of them, largely different to the fluids which would pass through the respective vessels, were these vessels dead. In this regard I would draw an analogy between the capillaries of the glomeruli of the kidney and those of the lung. Let us compare the uriniferous tubules to the air passages, and the dilated extremities or glomeruli to the lung alveoli. In the walls of the lung alveoli the capillaries are somewhat similarly situated to the tufts in the glomeruli. They are surface capillaries, being separated from the inside of the alveolus merely by the basement membrane and epithelium, just like the glomerular vessels—the epithelium being a thin single layer, just as in the glomeruli. But these pulmonary capillaries apparently allow next to no fluid to pass through them, and certainly no albuminous fluid. Again, the aqueous humour of the eye, according to the observations of Leber,* fills the anterior chamber at a pressure during life equal to 200 mm. (or 8 inches) of mercury, but it does not penetrate the substance of the cornea. Let, however, a portion of the thin layer of epithelium forming Descemet's membrane, which separates the aqueous from the cornea, be removed and the fluid at once penetrates. After death this penetration occurs regularly and the cornea becomes turbid and flattened, the pressure by the aqueous no longer keeping it convex, as it is lost by diffusion of the fluid. The living epithelium completely controls the fluid.

Similarly the vessels of the glomeruli, partly no doubt by virtue of the layer of epithelium which covers them, exercise a selective power, and do not allow the albumen to pass. And here I would note a point which is of some importance. The epithelium of the capsule does not merely line the inside of the capsule, but also covers the tuft of vessels. The capsule is formed in the fœtus by the invagination of the vessels into the blind end of the uriniferous tubule, and the tuft carries before it a layer of basement membrane and epithelium. Here is the description which Heidenhain† gives of this layer. "The loops of vessels in the tuft are covered with flat nucleated cells, and these do not merely form a continuous layer on its surface, but, penetrating into the tuft, cover its lobules and even the individual loops—a fact which has been often doubted, but is now perfectly established. The glomerular epithelium is most easily seen in the embryo and newly born, where its elements are cubical cells, having not yet become flat; but it can, even in the adult, be isolated as a continuous layer whose under surface shows concave depressions which are the moulds of the vessels of the glomerulus." Before the fluid from the vessels can reach the interior of the capsule, therefore, it must, after traversing the capillary wall, pass through the basement membrane and epithelium, and these may, along with the walls of the vessels, exercise a certain selection. In fact, this process approaches in reality to a secretion, and is not altogether a mere filtration. It is, then, the constitution of the separating membrane comprising vessel wall, basement membrane, and epithelium which prevents the passage of albumen.

* Leber, *Arch. f. Ophthalmologie*, xix.

† Heidenhain, in Hermann's *Handbuch der Physiologie*. Vol. v, p. 296.

It is generally agreed that it is chiefly water and salts which pass through the vessels of the glomeruli into the capsule which the dilated end of the uriniferous tubule forms, and that the urea, uric acid, and other special constituents of the urine are selected from the blood or lymph by the epithelium of the convoluted tubules. This view which is identified with the name of Bowman, although for some time dominated by Ludwig's theory, is now generally accepted, especially since it has been so powerfully advocated by Heidenhain. In relation to the present discussion, however, this is of less consequence, and I will pass on to two other matters connected with the circulation in the kidney, which have some relation to what comes after; these are the conditions of the circulation in the so-called arteriolæ rectæ on the one hand, and the capillaries on the other.

The arteriolæ rectæ are bunches of straight vessels, which have more the structure of capillaries than of arteries, and which, in the pyramidal portion of the kidney occupy a somewhat similar position to that of the glomeruli in the cortical part. From the main arteries running in the intermediate zone between pyramid and cortex, come off vessels which break up into these straight vessels, which are also reinforced by branches from the vasa efferentia of the deeper glomeruli. Now the blood in these arteriolæ rectæ, at least those directly connected with the main arteries, must like that in the glomeruli be at a high pressure and circulate at a high rate of speed. These vessels compare in another respect to those of the glomeruli, inasmuch as they also afterwards break up into capillaries just as the efferent vessels of the glomeruli do. I think that these similarities probably indicate a similarity in function, and that from these groups of vessels watery fluid probably passes through to be taken up by the neighbouring uriniferous tubules. It is true that the arteriolæ rectæ are in bundles and the central ones are separated from the tubules which run parallel to them, but fluid exuded will penetrate by the lymph spaces outwards to the neighbourhood of the tubules, especially as it will be at considerable pressure. If they possess this function, it will, however, be much subordinate to that of the glomeruli. We shall see afterwards that this course is probably taken by the fluid of the blood in some cases of Bright's disease.

In regard to the capillaries, it is generally held that, the blood, having been concentrated by losing its water in the glomeruli, it becomes again to some extent diluted in the capillaries by reabsorption of water from the tubules. It may here be pointed out that the blood in the capillaries probably circulates slowly. It has already passed through a capillary system in the glomeruli or arteriolæ rectæ, and the capillary system is so exceedingly rich that its aggregate capacity must greatly exceed that of the vessels of the glomeruli, and this also implies a retardation of the flow. The efferent vessel of the glomerulus being usually smaller than the afferent, there is a still further retardation of the stream as it passes to the capillaries. The circulation being thus slow there is time for interchange between the tubules and the capillaries, and there is time for the epithelium to attract the urinary constituents. In the separation of these constituents there may even be a passage of water from the capillaries into the tubules, and this will occur the more readily, if for any reason the separation of water at the glomeruli is diminished. In this regard some experiments by Nussbaum* are of great interest. His papers I have not been able to obtain, but from frequent references to them in other works, the following may be taken as the main points. Bowman† showed that in the boa, the kidney like the liver is supplied by two systems of vessels, the renal arteries, and a renal portal vein. This is the case with fishes, batrachians and ophidians in general, and Nussbaum took advantage of it to make some experiments in the frog. The renal artery supplies the glomeruli, while the renal portal vein supplies the capillaries which surround the tubules. These capillaries are also partly supplied by the efferent vessels of the glomeruli, and by direct arterial twigs. It is obvious that in these animals the ligature of the renal arteries will have the effect of shutting off the blood from the glomeruli, while blood will still pass by the portal vein to the capillaries around the tubules, although in diminished quantity. When this is done in the frog the secretion of urine ceases, even when the animal is placed for a considerable time in water, which otherwise has the effect of greatly increasing the flow of urine. But if, in such frogs a solution of urea be injected, the bladder fills with urine in the course of two or three hours. These experiments seem unequivocally to indicate that, while the uriniferous tubules take little part normally in the separation of the water of the urine, yet when they have exceptional duty to perform, as in separating large quantities of urea, water is separated by them.

* Nussbaum, Pflüger's *Arch. f. d. ges. Physiol.* Vols. xvi, 1878, and xvii, 1879.

† Bowman, *Phil. Trans.* Vol. i, p. 64, 1842.

Turning now more specifically to the subject of albuminuria, I would notice in the first place that, as many observers* have now shown, egg-albumen, if it be present in the blood of normal animals, appears in the urine. If it be injected into the blood or under the skin, or if it be present, unboiled, in large quantities in the stomach, then it will appear in the urine. And this escape of egg-albumen occurs at the glomeruli. In Nussbaum's experiments, if he injected egg-albumen as well as urea after ligaturing the renal arteries none of the former appeared in the urine. Ribbert† also found in warm-blooded animals that when egg-albumen was injected, and the kidney examined after death by boiling it so as to coagulate the albumen, the coagulated albumen was always found in tubules and glomeruli together, that is to say, if present in tubules it was also present in the corresponding glomeruli. We may say, I think, therefore that the glomeruli, while they do not allow serum albumen to pass, yet permit egg-albumen.

In regard to the separation of the serum albumen the first observation which I shall make is that, immediately on the death of the kidney, it loses altogether its power of preventing albumen escaping in the urine. According to Bidder‡ when the kidney is excised and blood carefully injected by the arteries, an albuminous fluid like the blood serum appears in the ureters. Apparently the vessels of the dead glomeruli becoming simply membranous tubes, allow the blood serum to transude through them without selection. We may perhaps infer that, the more the glomeruli are altered in the direction of dead matter the more will they permit the passage of albumen and even of other constituents, but it will be necessary to consider each pathological condition separately. It may here be noted that according to Ponfick (quoted by Heidenhain) hæmoglobin when present in the blood is separated by the epithelium of the tubules and not by the glomeruli.

Amyloid disease implies the replacement of the structures affected by it by a homogeneous structureless substance, possessing little evidences of vitality. If not actually dead and inert matter it must approach to that condition. In the kidney the structures primarily involved in this disease are,

* Cl. Bernard, Stockvis, Lehmann.

† Ribbert, *Centralbl. f. d. Med. Wiss.*, 1879, p. 836.

‡ Bidder, *Beiträge zur Lehre von der Function der Niere*, 1862 (quoted by Heidenhain.)

the smaller arteries, the vessels of the glomeruli, and the arteriolæ rectæ. A well stained specimen of a fully developed amyloid kidney presents very much the appearance of an injection of the arterial system. It is well known that polyuria and albuminuria are the most typical signs of amyloid disease of the kidney, and we may perhaps say that the degenerated Malpighian tufts and arteriolæ rectæ permit albumen and an excess of water to pass through. Albumen is almost constantly present in the urine in cases of amyloid disease, and the quantity is usually large (about 2 per cent according to Leube*). In a few slight cases it has been found absent, but in these the glomerular vessels may not have been sufficiently affected to lose their function.

In the next place, there are certain VASCULAR DERANGEMENTS which lead to albuminuria—namely, obstruction of the veins on the one hand, and of the arteries on the other. The most obvious effect of VENOUS OBSTRUCTION is rise of blood pressure, and that has usually been assigned as the cause of albuminuria in such cases. But so far as I can find, there is no unequivocal evidence that a simple rise in blood pressure if unaccompanied by stagnation of blood produces albuminuria. In all cases of excessive secretion of urine there is presumably rise of blood pressure in the glomeruli from dilatation of the arteries, an arterial congestion, but in such cases, unless there is some complicating element, no albumen appears in the urine. In venous congestion there is obviously stagnation of blood as well as rise in pressure, and the effect is the passage of albumen along with the water of the urine. In the congested kidney in man Langhans † has found slight enlargement of the epithelium of the glomeruli, and the glomerular vessels with the arteriolæ rectæ are the most prominently over-filled of the kidney vessels. But we are not to infer that all the albumen which appears in the urine comes from the glomeruli. As the obstruction is on the venous side the capillaries will be more directly disturbed than the Malpighian tufts, and transudation from them may pass into the tubules. Some observations by Senator seem to establish this. He produced venous engorgement by ligaturing the renal vein, but only allowed it to continue for 10 or 12 minutes when he quickly removed the kidney and put it at once in boiling water so as to fix the albumen. He found albumen in some of the straight tubules, especially of the medullary portion, but none in the glomeruli. If the experi-

* Salkowski and Leube, *Die Lehre vom Harn*, p. 367.

† Langhans, *Virchow's Archiv.* Vol. 76, p. 85. 1879.

ment were continued, then albumen appeared inside the capsules, and the longer it was continued the greater the amount of albumen there. We may infer, I think, that in venous congestion the albumen comes from the vessels around the tubules, and probably from the arteriolæ rectæ which are specially dilated in passive hyperæmia, as well as from the glomeruli.

A good many experiments have been made in regard to the effects of OBSTRUCTION OF THE RENAL ARTERY, of which the most interesting for our purpose are those of Overbeck (quoted by Heidenhain, Cohnheim, &c.) This observer ligatured the renal artery for a short period, only about a minute and a half, and then let go the ligature, so as to restore the circulation. The result of this was that the secretion of urine ceased for a time, sometimes as long as three-quarters of an hour, and when it returned the urine was highly albuminous. By degrees the flow increased and the albumen diminished. It does not need a complete closure of the renal artery to produce albuminuria, but, as Hermann has shown, a narrowing of it, sufficient to diminish the flow of blood, is sufficient. In these experiments we may presume, I think, that the deprivation of arterial blood, even for a short time, is sufficient so to disorder the delicate structures of the kidney, that when the circulation is restored, at first the vessels of the glomeruli are not sufficiently filled to cause water to pass, and then having partially recovered they allow albumen as well as water to get through. These experiments are of interest in relation to some of the forms of albuminuria in the human subject. Cohnheim refers to the fact that after the suppression of urine in cholera, due presumably to a stoppage of the circulation from the concentration of the blood, the urine is for a time albuminous. Then after epileptic and tetanic convulsions the urine is frequently albuminous. In these cases the general muscular spasm is accompanied by a spasm of the muscular coat of the arteries, as evidenced for instance by the blanching of the skin in epilepsy. This spasm of arteries extending to the kidney may be sufficient to cause stoppage or partial suspension of the circulation and suppression of urine, and when the arteries relax, the first urine is albuminous. Similarly we may, I believe, have spasm of the arteries and suppression of urine produced by reflex irritation as by passing a catheter or the impaction of a stone in the urethra. It is consistent with what we know as to temporary obstruction of arteries that even an acute inflammation may be set up in this way, and I think that some cases of so-called catheter fever may take their origin from reflex spasm of the renal arteries.

We come now to the subject of Bright's Disease, which affords the most frequent and therefore the most important cause of albuminuria. In discussing the causes of albuminuria in Acute Bright's Disease, I think the fact is not sufficiently borne in mind that we have here to do with an acute inflammation of a very vascular organ. Dr. Greenfield in his remarks on this subject has to some extent anticipated what I have written here, and it may be of some interest that two pathologists have come to what are in many respects the same results. Permit me, to begin with, to remind you of the principal phenomena presented in acute inflammations generally. There is at first dilatation of the arteries and an active congestion. But if the inflammation be at all intense this is followed by a slowing of the current especially manifest in the smaller vessels. I do not here go into this particularly, but every one who has studied the matter experimentally is agreed that in an acutely inflamed part the blood current in the capillaries is much retarded, and this may even approach the condition of stasis. I do not see how the kidney should form an exception to that rule, and I think we may assume that in the glomeruli, arteriolæ rectæ, and capillaries the blood is going at a slow rate, and is under a low pressure. This alone is sufficient to explain the scanty urine in acute Bright's disease. The amount of urine seems to depend on the one hand on the amount of blood which passes through the glomeruli in a given time, and on the other hand on the activity of the epithelium in removing the water from the urine as separated by the glomeruli. In the present case the epithelium can hardly be more active than usual, is probably much less so, being paralysed by the inflammation. I cannot agree therefore with Prof. Hamilton when he supposes that an excess of urine passes through the glomeruli in this disease. I believe that the urine is scanty because the blood current is slow as it is in inflammations in general.

But in acute inflammations in general we have the constituents of the blood leaving the blood-vessels, especially the capillaries and veins. That this is not a mere increase in the normal transudation, is amply evidenced by the case of inflammation of the lungs, and, from the resemblance in structure already referred to, I regard this analogy as peculiarly appropriate. In the normal lung there is no transudation from the capillaries into the alveoli, or at most a slight watery transudation. In acute pneumonia you have in the first stage the alveoli filled up with a concentrated albuminous fluid in which are suspended leucocytes and red corpuscles. All three constituents, albuminous fluid, leucocytes, and red corpuscles, are transuded through the walls of the capillaries and that without rupture of the latter. Let us carry this analogy to the kidney. From the blood stagnating in the glomeruli, capillaries, arteriolæ rectæ, and veins, albuminous fluid transudes, and the urine secreted is not a proper urine at all, but to a large extent an inflammatory exudation.

The question may here be asked, Is there any evidence as to the locality at which these constituents of the blood leave the vessels and pass into the tubules? The red corpuscles undoubtedly pass through the glomerular vessels, they are to be seen inside the capsules and in the convoluted tubules, sometimes even in a capsule and corresponding tubule. Albumen also passes through along with the red corpuscles just as it does in the lung.

As to the leucocytes there is very little mention of them at all in treating of Bright's disease, and yet I believe that these bodies, which are really the white blood corpuscles escaped from the vessels, are very commonly present in the urine in acute Bright's disease. Even well preserved leucocytes are frequently present in considerable abundance, and there are others less distinguishable, being altered by the fluid in which they find themselves. For several years I have not been satisfied that free desquamated epithelium is present so abundantly as is usually stated in the urine in acute Bright's disease. Any one who is familiar with the renal epithelium as seen in the kidney after death will have great difficulty in recognising as such what is commonly called epithelium in the urine. I believe that this is largely leucocytes, and that the presence of well preserved renal epithelium is very rare. I have never been convinced that the term desquamative nephritis is a just one, and although all pathologists are familiar with the appearance in the kidney of epithelium carried from higher parts of the tubules and impacted into lower, still this is always greatly altered epithelium which if it reaches the urine will appear not as well formed cells but as degenerated cells or mere debris.

In this regard I was considerably impressed with a case which I had the opportunity some time ago of observing both during life and after death. The man laboured under a sub-acute attack of Bright's disease which had lasted for about a year when he was admitted to the Western Infirmary under my care while acting for Dr. Gairdner. This man had had several dropsical attacks and had the peculiar anæmic appearance that one usually associates with acute Bright's disease. The urine was loaded with albumen, but it contained in addition immense numbers of leucocytes, so much so that the deposit had the general appearance and microscopic characters of pus, being noted as such in the urine book. This struck me very much at the time and I tried to discover whether any source of suppuration existed anywhere, but could find none. On examination after death it was perfectly apparent that the leucocytes came from the kidneys. The renal tissue was at intervals greatly infiltrated with round cells and they were found abundantly in the tubules as well as outside them. It was a case which might be called acute interstitial nephritis, but had all the clinical features of sub-acute Bright's disease, and I cannot see that in essentials it was different from that. An interesting enquiry was as to the exact source of the leucocytes and the path by which they reached the urine. From a careful examination I have concluded that although probably the majority yet by no means all came from the glomerular vessels. I certainly found some in the inside of the capsules, but I found also the following very suggestive appearance. In the medullary portion I met occasionally with arteriolæ rectæ packed with leucocytes, and uriniferous tubules alongside them also visibly containing them. Clearly the white corpuscles had accumulated in these vessels as they do in the veins and capillaries in inflammations, and were being exuded, many finding their way into the tubules. I would say that in this case a passage of leucocytes was taking place more or less from all the capillary vessels of the kidney into the tubules, and I have no doubt that along with them albuminous fluid was exuding. This case may indeed serve as an indication that in cases where the passage of leucocytes is much less marked, albuminous fluid makes its way by the same paths, and that it also does not come exclusively from the glomeruli. It will be remembered that this conclusion was also come to, on a different basis, in the case of venous congestion.

I would here allude to the fact recorded by myself as well as other observers, that in scarlatina there is often an excessive exudation of leucocytes visible in the kidney after death, a condition closely resembling that in the case I have described. I don't know that these cases differ clinically from those in which there is no such excessive exudation of leucocytes, and it seems to me to be probable that circumstances which we do not understand determine whether these shall or shall not be an excessive transudation of white corpuscles. I would here suggest that in examining scarlatinal urine more attention should be paid to the presence of leucocytes. To return to ordinary parenchymatous nephritis it may be stated as a fact, that when cases of acute Bright's disease have lasted for any considerable time, whether primarily scarlatinal or not, there are always round cells abundantly present in the kidneys. This also is a point which Dr. Greenfield asserted.

We now come to the very peculiar case of chronic Bright's disease, the typical anatomical condition of which is the contracted kidney. As just indicated, I agree with Dr. Greenfield that no sharp line of distinction is to be drawn between parenchymatous and interstitial nephritis, but for our present purpose the condition of the urine in cases which have a chronic and insidious course from the outset is of interest as contrasted with the acute cases already considered. What strikes one very forcibly in this condition is that the state of the urine contrasts very forcibly with that of the kidney itself. The urine is copious and watery, and though the percentage of urea as of all the dissolved constituents is small, yet the total amount in the twenty-four hours is not usually below the normal, although the danger of the excretion of urea becoming deficient is the chief one in this disease. While the urine is copious, and on the whole not deficient in urea, there is little or no albumen, it is a very watery urine but otherwise little removed from normal. And yet the kidneys are very greatly altered; there is great destruction both of glomeruli and tubules. The glomeruli are very remarkably destroyed in many cases, being converted into solid bodies in large numbers and only a few remaining at all injectable. The convoluted tubules are even more destroyed, and in many cases it is quite remarkable in microscopic sections to see how closely set the glomeruli are from destruction of the intervening tubules. In addition there is considerable destruction of the capillaries, which are, along with the tubules, enclosed in new formed connective tissue and pressed on by the contraction of this tissue. It is clear that there is great atrophy of the structures which separate both the water and the other constituents, and that whether you take one view of the process or another. The structures which survive must obviously do as it were double duty, and so far as the separation of water is concerned, more than double. Taking in the first place the polyuria, the state of the circulation in the kidney is plainly such that an excessive amount of blood and at an abnormally high pressure will pass to the surviving Malpighian tufts. The majority of the tufts are destroyed, their vessels occluded, and this itself will throw an additional pressure on the surviving ones. It is to be presumed that more water passes there than normal, but it can hardly be equal to what would pass were all the glomeruli open. There must be some interference with the absorption of the water, and I think we may say that the urine as it is passed into the bladder is very much like that which comes from the glomeruli. The explanation of the non-absorption of the water is evidently the destruction of the renal epithelium and of the capillaries. The remaining tubules and capillaries are probably too actively concerned in the removal of the proper urinary constituents to do much in the absorption of water. A time comes by and bye when the atrophied renal structures are no longer equal to their duty, and the urinary constituents being retained in the blood uræmic symptoms appear. As to the albumen in this disease, we are not surprised at its small amount. It is a chronic inflammation, and there is little inflammatory exudation in most cases. In particular, the glomerular vessels are not directly affected and their epithelium is not specially involved. I confess that the small quantity and often the absence of albumen in these cases has always seemed to me entirely to refute the view that albumen is separated with the water in the glomeruli. Clearly the water is not reabsorbed after the glomeruli are passed, and that because the renal epithelium and capillaries are destroyed. For the same reason the albumen, were it there, would not be reabsorbed and would appear in the urine. But the albumen is not only small in percentage but also in the total amount lost in the 24 hours.*

* It is customary to say that in chronic interstitial nephritis there is such thickening of the internal coat of the arteries (endarteritis obliterans) that, these vessels being greatly narrowed, any increase of blood pressure in the systemic arterial system will not be propagated to the arteries of the kidney. No one who is in the habit of examining the kidney will deny that there is such narrowing of the renal arteries, especially the smaller ones, but it is to be remembered that there is also great destruction of glomeruli and capillaries, as well as of the other structures of the cortex. The question comes to be therefore whether the arteries as a whole are wider or narrower than normal in relation to what remains of renal tissue. This question has been subjected to the test of very careful measurements by Thomas. (*Virchow's Archiv*, vol. 71, p. 42, 1877.) He finds that in relation to the weight of the kidney, the arteries are distinctly wider than normal.

It is difficult from the very imperfect summary which Dr. Mahomed gave here, and has published, of his longer paper, to be quite sure of the exact character of his views. I understood, however, that the chief cause of albuminuria in his view, is increased arterial pressure, and probably he was here referring especially to the case of chronic Bright's disease. Now the whole bearing of my remarks hitherto has been in the direction that more is needed in the production of albuminuria than mere increase of arterial pressure. I would point out more particularly that in the disease under consideration, even accepting his view in general, facts do not bear it out. Albumen is much more apt to appear when the pressure is low than when it is high. The excess of water in the urine in this disease is, as we have seen, in part related to the excess of pressure and speed of the blood in the surviving glomeruli. But it is, I think, an acknowledged clinical fact that, in these cases, it is when the amount of urine is reduced that albumen is most apt to appear in the urine. The reduction of the amount of water in the urine indicates diminished action in the glomerular vessels, and it is then that the albumen appears. According to the view which I have been advocating the diminution in the amount of urine is probably due to a slight exacerbation of the inflammation, and this also explains the increase in the amount of the albumen.

It is not necessary for me to go through the other conditions in which albuminuria occurs, and endeavour to explain their pathology. Dr. Finlayson, in his admirable remarks referred to most of these conditions, such as acute fevers, &c., and, reducing his remarks to pathological terms, I would say somewhat as follows. Whenever we have in the blood a morbid poison which specially attacks the renal structures, then we are liable to have albuminuria, and this is the more likely to occur in the case of those poisons which, like cantharides, are eliminated by the kidneys. In these cases as in the other it is not a case of variation in the blood pressure but of damage to the blood-vessels and other structures in the kidneys.

And now in conclusion it seems to me that albuminuria implies the abnormal passage of the serum albumen from the blood through the blood-vessels into the urine. This is due in every case to an alteration in the constitution of these vessels, inflammatory or other. It is not to be inferred that the albumen comes alone from the vessels of the glomeruli, but it may do so from the arteriolæ rectæ and the capillaries, although in the latter case at least there is, normally, no considerable passage of fluid from the blood to the interior of the tubules.

Albuminuria from the Physiological Point of View.

By Dr. M'GREGOR-ROBERTSON.

Mr. President and Gentlemen,—I venture to think that the key to the solution of this problem is a physiological one. I would not dare to offer any remarks on the pathological states, or the clinical aspects of this disease, but it seems to me needful to ask renewed attention to the physiological view point in the hope of getting fresh light thrown on the subject under discussion. Dr. Newman has already referred to the physiological conditions, and perhaps is the only one, of those who have spoken on the subject, who has referred in any detail to them. It is impossible to understand, I would suppose, the causes of an interference with the normal function of the kidney unless you understand what that normal function is, and still less is it possible to understand the nature of the interference without a previous acquaintance with the healthy condition.

Now, as histology indicates, there are manifestly two parts in the process of secretion by the kidney; there is the part played by the glomerules, and the part played by the renal epithelium. As regards the part played by the glomerules, it is universally admitted to be a process of filtration. The mere histology of the organ—the tuft of capillary vessels with large ingoing and small outgoing vessel, surrounded by the dilated extremity of the tubule —suggests the filtration process. I observe that Dr. Coats objects to the phrase "filtration" altogether; I was under the impression that it was universally admitted to be a filtration process. The filtration being admitted, the only question is regarding the nature of the filtrate. It is round this question that the controversy is first waged, principally in the effort to determine whether albumen is or is not present in the normal filtrate. It seems to me that the whole difficulty, or at least a large portion of the difficulty, that has arisen in regard to whether albumen passes through the capillary walls into the dilated extremities of the tubules or not is due to a failure to appreciate what filtration actually is. Yet filtration as a physical process is well understood, and the facts and laws of the process have been laid down as early as 1856. How the confusions and differences of opinion have arisen may be understood if we take as an example of them that afforded by some remarks made by Dr. Mahomed.

Dr. Mahomed passed over the physiological part of his paper, observing that he was content with the observations that had been made by Dr. Newman and others. He accepted, that is, the filtration view, and yet "it was pointed out" (I quote from the report of his paper) that while water might readily transude from the glomeruli under normal pressure, albumen *being less diffusible* (the italics are mine) would probably be retained, but that slight delay and increased turgescence in the glomeruli might permit of the exudation of paraglobulin and other more diffusible forms of albumen, before the appearance of serum albumen." That sentence shows conclusively that Dr. Mahomed, as far as his argument is concerned, has confused entirely the process of filtration and the process of osmosis. These two processes, it has been experimentally proved, are essentially different from one another. What osmosis is, at least so far as its results are concerned, the researches of Liebig and of Graham have determined. Graham showed that if a solution containing crystalloids and colloids be separated from water by an animal membrane, the crystalloids will rapidly pass through the membrane, and gain access to the water, but the colloids, because of their extremely small diffusive power, will be retained. If, then, you call the process in the glomerular tufts one of osmosis, the retention of albumen and the passage of salts is readily enough accounted for. But all the conditions in the Malpighian bodies are opposed to that view of the process, and if it be Dr. Mahomed's view, he is, I think, singular in maintaining it. Filtration, on the other hand, is of an essentially different nature. By it is meant the process by which fluid is forced *through the pores* of a membrane under pressure. As Liebig showed, you may have a membrane in which it may be asserted no pores exist, such as a septum of coagulated albumen, in which, therefore, the conditions for filtration do not exist, but through which osmosis may readily go on.

As showing how the confusion between the two processes prevails in Dr. Mahomed's paper, I may recall the statement —I do not find the exact words—that various forms of albumen may be found in the urine, some of them earlier than others. He thinks that paraglobulin being more easily *dialysed* than serum albumen will explain how it often appears earlier than serum albumen. Therefore his declaration that serum albumen will not pass through the glomeruli is based on the law of osmosis, but is wholly inapplicable to a case of filtration, which is said to occur in the glomeruli. Now, the laws and nature of filtration have been as clearly laid down as those of osmose. In Poggendorf's *Annalen* for 1856 (page 337), there is a paper by Dr. Wilibald Schmidt of Voigtland "On the speed of filtration of different solutions through animal membranes," and in the volume for 1861, (p. 337) another paper by Schmidt "On the character of the filtrates in the filtration through animal membranes of solutions of gum, egg albumen, common salt, urea, and nitrate of potash." Previous researches by Von Wittich and others had made it probable that the fluid on the filter and the filtrate "remained of similar qualitative composition." In other words, that the composition of the two fluids is identical as to the substances they contain, and they differ only in the quantity of the substances in each case. Schmidt shows that various substances differ in their rates of filtration, that salts filter faster than albumen, and also that albumen present in a saline solution will filter less readily than it would do were it the only substance in solution. The presence of salts impedes its filtration. In no case, however, was the filtration of albumen completely arrested. A certain amount always did pass through the membrane.

If, therefore, you call this process going on in the Malpighian body of the kidney a filtration—and I believe it is universally admitted that it is a process of filtration—you must accept the facts of that physical process, and you have no rational ground for assuming that albumen is retained by the walls of the vessels or by the epithelium of the glomerulus. You must, then, come to the conclusion that all the normal constituents of the blood plasma transude through the capillary walls into the cavity of the glomerulus, though in vastly different proportions from those in which they exist in the plasma, the albumen—because it filters less easily, and because of the saline nature of the blood—being specially diminished, but nevertheless present, in the filtrate.

These physical facts are admitted to a large extent in the theory of Bowman as well as in that of Ludwig. But because albumen does not appear in normal urine, some condition of the glomerular or capillary walls is imagined to prevent the passage of that substance. The name of filtration is given to the process, but the essence of it is denied. This arises from the difficulty of understanding what becomes of the albumen, admitting it to be transuded. It is here that the second element in the function of the kidney enters—viz., the activity of the cells lining the tubules.

The theory, which was mentioned by Dr. Newman, is a theory which has been started by a very considerable number of physiologists, although it does not appear to have found a place in our English text books. The view that Dr. Newman upheld was that the albumen is absorbed by the renal epithelium. It has occurred to me that, if you accept this view, some explanation may be offered of the differences in the diameter of the renal tubules. The fluid which has been filtered into the tubule is at first, comparatively speaking, rich in albumen. It passes at once into the convoluted tubules, and, as the fluid moves along, the cells have no difficulty in absorbing the albumen. But when the urine reaches the loop tubules of Henle the amount of albumen it contains has been greatly diminished, and if it passed along tubes of the ordinary diameter the albumen would escape absorption. The contracted diameter of the loop tubules of Henle, however, delays the passage of the urine and gives plenty of time for the active cells which line one limb of the tube to absorb from it the remaining amount of albumen. Now, so far, it is mere theory that the cells absorb the albumen; but within the last two or three weeks, I think I have been able to discover a fact which affords a strong confirmation of the view I have taken. It is known that if atropine be injected into the veins of an animal the salivary secretion will be arrested although the chorda tympani be stimulated. That is to say, in spite of an increased quantity of blood flowing through the glands there is no secretion. The conclusion is obvious, that that arrest of the activity of the salivary glands is due to a paralysis of the cells of the gland. We know, also, that atropine hypodermically injected arrests the secretion of the sudoriparous glands. We know, from histology, that the nature of the structure of the glands is not one which favours filtration, but which favours the separation by the active cells. Therefore, we conclude, that here also atropine paralyses the gland cells. Atropine, however, is stated to have no influence on the renal secretion, and that fact is advanced as a strong proof of the urine being a filtration and not a secretion by cells.

Now, I argue in this way. Undoubtedly atropine cannot affect the *quantity* of the urine if it is a filtration. Supposing it influenced the kidney at all, from the analogy of the other glands, it would act on and paralyse the renal cells. If they were paralysed they would be unable to absorb the albumen, which would, therefore, appear in the urine. I put the view to the test of an experiment. I injected a quantity of atropine under the skin of a cat, which I had kept for a few days, and whose urine I had tested and found free from albumen. The first urine which was passed some time after, showed a distinct quantity of albumen. After some further waiting the next quantity of urine showed an increase in the quantity of albumen. Albumen appeared up to the second day after the injection, and on the third day it had entirely disappeared, on which day also the cat seemed quite recovered from the toxic influences of the drug.* I think this experiment affords strong evidence of the view that the renal epithelium absorbs the albumen filtered into the capsule of the tubule.

Now, from this physiological view point, what are the conditions liable to produce albuminuria? The conditions have been, to a considerable extent, mentioned by Dr. Newman. Taking a very general view, the first condition is increased pressure in the glomerules increasing the quantity of albumen in the filtrate. This also is in accordance with the results of Schmidt. The quantity may be too great for the epithelium to absorb, and the excess will appear in the urine. Then if the renal epithelium be disorganised, even though there be no additional amount of albumen filtered through the glomerulus, the cells will fail to absorb even the normal amount, and again the albumen will appear.

I venture to think the physiological view I have taken will afford an explanation of the instances of temporary albuminuria, mentioned by Dr. Mortimer Granville, and will throw some light on the so-called physiological albuminuria. Dr. Mortimer Granville has shown that in many cases where there is no apparent disease of the kidneys albumen may appear in the urine, and he says that in such cases the standard of health is commonly below par. In fact, the conditions are those of anæmia and those of exhausted nerve power. Under these circumstances you would not be surprised should there be diminished functional activity of renal epithelium as of other structures. This diminished activity might prevent the renal cells from discharging their normal function to the full; some albumen would escape them and appear in the urine.

* I am continuing my observations on this subject, and hope to be able to publish some results in the course of a short time.

Thus, by a comparative degree of renal sluggishness, so to speak, without any pathological change a temporary albuminuria would occur, to disappear as soon as natural tone was re-established.

Then, as to the "physiological albuminuria," I confess from the view I have taken you might as well speak of physiological dyspepsia. For you might have diminished functional activity of cells of the gastric glands, due to lessened blood supply or want of normal nervous influence, resulting in diminished vigour of the gastric juice and deficient gastric digestion—in short, resulting in dyspepsia lasting just so long as the natural activities of the gastric glands were below the average. Precisely so you might have in the kidney the renal epithelium being deficient in the discharge of its functions and causing the temporary appearance of albumen in the urine.

Thus, I venture to think, if we can unravel the physiology of the renal secretion we shall gain valuable information for the understanding of abnormal conditions and intricate pathological problems.

On Albumen Tests.

By ROBERT KIRK, M.D.

Mr. President and Gentlemen,—The remarks I have to make to-night will be confined solely to the subject of albumen tests. In view of the clinical problems connected with the appearance of a trace of albumen in the urine—in view of such questions as those so ably raised by Drs. Roberts, Mahomed, and others, in this discussion, it is evident that the reliability of our tests is of the first importance. Some of the points involved turn on the detection of a small quantity of albumen, and if one finds this where another does not they cannot debate the question of its significance on common ground. I have paid considerable attention to these tests, and hope to enlist your interest in them.

The result of Dr. Roberts very careful investigation is that we should abide by the old tests, heat and nitric acid. It is worthy of remark that the search after new tests was largely instigated by Dr. Roberts himself, so that credit would be fairly due to him for any superiority they might possess. In his well known work on *Urinary and Renal Diseases* he pointed out the inferiority of nitric acid, and suggested that a more delicate test than any we yet possessed was a desideratum of some importance. I was one who engaged in the search about four years ago. In the course of an examination of the reactions of albumen I took up the subject where Dr. Roberts left it in his work. In this I was assisted by an ingenious friend, the Rev. Mr. Gibson, of Partick, who hit on the method of applying picric acid on the surface of the urine. The delicacy of the test in this way I demonstrated in this hall in 1881. I afterwards found, from the last edition of Neubauer and Vogel, that shortly before it had been employed in a somewhat similar, but not identical way, by Hager. It is true that the test had been known for some years previously, but owing to a faulty method it was not superior to other tests. Since that time Dr. George Johnson has succeeded in awakening increased interest in this test, and it seems to be now somewhat extensively used. Other new tests have also been recently recommended, and the question I wish to lay before you is, whether these possess any advantages over the old.

At the outset, I readily admit that heat and acetic acid is a very delicate test, but I feel bound to say that I regard Heller's nitric acid test as hopelessly inferior. Still worse, however, is the practice of acidulating with strong nitric acid in using the heat test. This is still mentioned in some works, and it is now recommended in the latest editions of Neubauer and Vogel in preference to heat and acetic acid, on account of the difficulty arising from the mucin reaction of the latter. Now, so complex are the reactions of albumen with heat and nitric acid, varying as they do with four several proportions of acid, with certain states of the albumen, with the amount of salts and other crystalloids in the urine, that it would be far too tedious to discuss this question here, and I would only record my opinion that scarcely any amount of experience is sufficient to guard against error, and that more serious mistakes have arisen from this practice than from all other causes put together. Let us confine our attention, therefore, to Heller's test. It is easy to show that with this test we may fail to detect albumen even when present in quantities, which can scarcely be described as mere traces—quantities of clear pathological significance. I believe it will soon be discontinued. No doubt it has the merit of being very uniform in its action, and when it gives a positive result is thoroughly reliable; while we are so familiar with it that we feel as if we at once attached the proper clinical significance to the results we obtain with it.

In trying the new tests most will feel at first the want of the old landmark, and will find it necessary for a time to refer to it for comparison; and justly so. It is only when it gives a negative result that we cannot rely on it, and of this I hope to convince you.

The proof is twofold. In the first place, an albuminous urine can be treated in such a way as to render nitric acid much more delicate. If you make the urine tolerably alkaline, and allow it to stand twenty-four hours, or boil it and allow it to cool, you will find that nitric acid will detect a trace of albumen which it failed to do previously. If you keep such an albuminous urine a still longer time (for several days), even although it be acid, you find a similar result. Only a few weeks ago I was testing the urine of a patient recovering from acute nephritis. In view of the present meeting I compared nitric acid with other tests in this and other cases. In this case it gave no reaction at all in twenty minutes, and only a very slight and doubtful one in half-an-hour.* Other tests, however, unmistakably indicated a trace of albumen; but as the patient was taking quinine, so that picric acid could not be used in the most delicate way, and as the urine contained also more than the usual amount of mucin, it appeared desirable to test the matter more thoroughly. I added some salt and acetic acid to the urine and set it aside. In a day or two there was a considerable deposit of mucus at the bottom, but I did not test again for albumen for five or six days. Not only was there now reaction with other tests as before, but nitric acid also gave a decisive result in a single minute. I have repeatedly seen instances of this.

The second proof is the superior delicacy of other tests. In this comparison three points require attention. We must first observe carefully the effect of any new test on normal urine. We then add a few drops of an albuminous urine to the normal secretion and note when the albuminous reaction appears, leaving a wide margin between it and the effect on normal urine. It then remains to distinguish between the former and all possible reactions occurring in any urine whatever. The latter is evidently a difficult point with any

* Hitherto it has not been thought necessary to wait more than five minutes or so with nitric acid, and it was even thought by some more delicate than heat; the proposed extension to half-an-hour is a clear tribute to the merit of more delicate tests. But even half-an-hour does not put it on a par with the latter, and it is evident that formerly traces of albumen must have been undetected by it.

test, and we cannot expect it to be at once and easily settled. Let us now take one test, as picric acid, and compare it with nitric. Here it is well to distinguish between two methods of testing: the ordinary one, in which the urine and test fluid are mixed, and that in which they form two separate layers, as in Heller's method with nitric acid. The latter Dr. Oliver has appropriately called the "contact" method, and we may speak of the other as the "diffusion" method, for the terms are exceedingly convenient. The contact method gives us a great increase of delicacy, whatever the test, and to obtain the maximum with picric acid it is necessary to run the solution with the greatest gentleness over the surface of the urine by means of a glass tube. In this way we find that picric acid detects in a minute a trace of albumen, which nitric acid altogether fails to reveal. Further, the test shows that a quantity of albumen, which formerly would certainly not have been detected by nitric acid, is scarcely such a small trace as you might suppose. I show you an illustration of this. Here is a urine containing a trace of albumen, just enough to give a very faint reaction with nitric acid in ten minutes. Here are five test tubes, showing the albuminous zone produced with five other tests—picric, chromic, and metaphosphoric acids; potassium ferrocyanide and sodium tungstate. These several tests have all been applied without admixture on the surface of the urine, and the results are the dense and distinct zones, perfectly defined above and below, which you see in these tubes, and which will remain there unbroken for hours. If you consider that the tolerably large precipitates have been formed by the mere contact of the urine and test fluids over an area only three-fourths of an inch in diameter, you will see that the entire amount of albumen in the secretion of 24 hours, perhaps 60 or 80 ounces, must be considerable. The specimens illustrate how you ought to test when you aim at the greatest possible delicacy —namely, without admixture, so as to get the well defined zones you see, the naked eye characters of which are sufficient to distinguish them from some other reactions, as for instance that of urates with the several tests. I venture to think that you have but to see the beauty and delicacy of those tests to become enamoured of them.

With regard to the alleged reactions of picric acid with normal urine I have found it less objectionable on this score than almost any other test I know. Used as I have described, it generally gives absolutely no result with normal urine, or, indeed, with any non-albuminous urine, unless we watch for more than five minutes, when it may produce a slight result with urates and mucin; but it is quite unnecessary to continue the observation beyond one or two minutes. If there is no result in that time we may negative the presence of albumen in the most decisive way. There are one or two other points here worth mentioning. In making a saturated solution of the acid by pouring boiling water on the crystals it is well to let it stand a whole day or more to allow the excess to crystallise out, so that there may be no precipitation afterwards. I find it a good plan to add one ounce of water to nine of the saturated solution, which involves no loss of delicacy, and prevents any turbidity at the line of contact with the urine,—a turbidity sometimes due to the precipitation of free picric acid, which is necessarily less soluble in the urine than in pure water. Another point is to acidulate an alkaline urine with acetic acid before testing. It is true that if we test carefully without diffusion there is little or no danger from this cause, but it is well to guard against it. And if we test by mixing an equal volume of the saturated solution with the urine, according to one of Dr. Johnson's rules, it is essential to acidulate first if the urine be alkaline, for I have seen such urine require three or four volumes of the solution to precipitate the albumen.

But it is objected that, besides albumen, picric acid precipitates quinine, urates, mucin, peptone, and hemi-albumose. Now, we all know urates and mucin, but probably some will be disposed to ask, What is hemi-albumose? In the last edition of Neubauer and Vogel we find that this is the name now given to the body first found in the urine by Bence Jones in a case of osteomalacia, and originally called metalbumin. In Foster's *Physiology* it is stated that hemi-albumose is parapeptone, having the same reactions as acid-albumin, and that it is even precipitated by nitric acid. If so, therefore, surely when it occurs in the urine it is well to find it. To come to peptone proper: you know it is disputed whether the body which goes by this name in the urine is identical with peptone. I have examined a number of cases of peptonuria, which have fallen under my observation, and can only say that in all cases these urines contained ordinary albumen, precipitable by heat. I have never found cases of peptonuria fail to give as large a precipitate with potassium ferrocyanide as with heat, and, as you all know, peptone is not precipitated by the former. It so happens that only nine days ago I had under observation the most marked case of peptonuria I have ever met with. The urine in this case gave with heat an albuminous precipitate of half the tube, and contained, besides serum-albumen and peptone, a considerable amount of paraglobulin. When some of it was added to about twenty volumes of water the mixture became of a milky white, as you see in this bottle. The urine of this patient is now much less albuminous and does not show the same reactions well. But I can show you a specimen of the urine I got nine days ago, which has been carefully corked up, and it still shows the reaction mainly relied on as indicating the presence of peptone. This is a ruby-red colour above Fehling's solution in the cold, used as nitric acid in Heller's test; albumen giving a violet in the same way. This urine gives the red colour almost immediately; when the quality of albumen or peptone is small the colour takes some time to appear, and in the case of peptone may be only pink. It has been stated that peptonuria is common in amyloid degeneration of the kidneys. All the cases I have seen have been serious cases of albuminuria, and there is no evidence to show that any such body as peptone, precipitable by picric acid, has ever occurred in the urine in the absence of albumen.

A precipitate of urates is extremely rare, and after a time breaks up into an irregular form easily recognised by the naked eye. Quinine and mucin, however, but especially the latter, are more serious difficulties. It is true that the precipitates with both urates and quinine disappear with heat, but if we are obliged to have recourse to this in every case we lose the delicate method of testing, and (what is worse, and a fatal objection) we cannot exclude a possible mucin reaction with the acid in this way, for this is much intensified by heat. To exclude mucin, as I hope to show, we must retain the contact method, so that to exclude quinine at the same time we must have recourse to some other test equally delicate.

We have a number of such. Here let me mention a curious principle which I found to affect albumen tests; I refer to the influence of specific gravity on them. It has generally been thought that we could not have too much of the reagent; hence the invariable use of a saturated solution. But Mr. Gibson and I, from some of our experiments, were led to doubt this. Why, for instance, should the solution of picric acid be a delicate test? It is very slightly soluble, and the saturated solution contains, after all, only a trace of the reagent. If you make a strong solution in ether, in which it is very soluble, you find that you gain nothing, while, if you combine it with a heavy solution of metaphosphoric acid, you at once impair its delicacy. We had formerly experimented with a saturated solution of the latter and found it unsatisfactory. It now occurred to us to try a weak solution of specific gravity 1003 to 1005, and we at once found it to reveal a trace of albumen when the saturated solution altogether failed; it was, indeed, scarcely inferior to picric acid. Similarly with potassium ferrocyanide. Instead of using a saturated solution of this, strongly acidulated with citric acid, it is much more delicate to add a few drops to the urine, and to apply over the surface a solution of citric acid, of sp. gr. 1005. I found this also a convenient method of using another delicate test, sodium tungstate, introduced by Dr. Oliver, whose ingenious test papers, very satisfactory for clinical work, and which I have tried, must be invaluable to those who have to carry tests about with them. I should here observe, however, that Dr. Oliver's solution is equally delicate, as it is not too heavy. I also compared with the above the potassio-mercuric iodide test, but found it produce so much reaction in normal urine that I could not rank it with the others. Let me only mention one test more,—chromic acid. A solution of this acid, of sp. gr. 1005, on the principle I have mentioned, is extremely delicate, and quite equal to picric acid under all circumstances. It has no effect on quinine, and so far as I have observed does not precipitate anything which cannot be distinguished by the naked eye from albumen. A number of very careful comparisons showed that these tests were all very nearly alike. But it was found that when the specific gravity of the urine was high they all suffered in delicacy, except picric and chromic acids. Indeed, when salt was added to a urine of specific gravity 1020, so as to raise this to 1030, some of them were perceptibly affected. Picric and chromic acid, therefore, I could not but regard as the very best of the number.

Now, with picric acid, and one or two more of these tests, we may exclude every possible non-albuminous precipitate. If we take chromic acid and potassium ferrocyanide, urates and mucin are the only bodies they precipitate in common with picric acid. Mucin, indeed, is the only difficulty, but the following will be found a satisfactory method of dealing with it. A much better precipitant of mucin than any of the albumen tests is citric acid, and in accordance with the principle already alluded to, a solution of specific gravity 1005 is an excellent one for this purpose. Hence, test with this first and observe the effect for 2 minutes. Generally it is *nil*, but sometimes it is considerable. If mucin alone be present the effect with the albumen test is always less than with citric acid: but if there be a trace of albumen, the result with the former is much more rapid and decisive. If the two are equal (and I have seen a marked and identical reaction with both), we cannot be absolutely certain whether a trace of albumen may be present or not, and if it be thought necessary to proceed so far recourse must be had to means for isolating the mucin. These tests will almost invariably give a reaction with urates and mucin after a sufficient length of time—generally 5 to 15 minutes. Potassium ferrocyanide gives a very slight reaction with mucin, and none at all with peptone.

The heat test is no doubt very delicate, for I have carefully tested it, but I cannot admit that it is equal, much less superior, especially in the urine, to the tests above mentioned. Dr. Roberts has arrived at a different conclusion, and I do not doubt his facts, but there are two ways of making the comparison. It is necessary to adhere to the contact method of testing, and the experiment shows conclusively its superiority. When you dilute an albuminous urine with a large quantity of water the specific gravity falls so low that you cannot float the test solution on the surface, and if you now test by simple diffusion you may really find both picric and chromic acids inferior. But all that is necessary is to add a little salt to the diluted urine till the specific gravity rises to 1015 or more, and the contact method again becomes available. You then find that they are unrivalled.

But is the heat test free from fallacies? This is the important question. I believe the difficulty of proper acidulation is greater than is generally supposed. No rule on this point can be applied to all cases, as the proper amount of acid to use varies with certain states of the albumen, and, in addition, with the specific gravity of the urine. The latter point is of great practical importance, and here is to be found the explanation of the fact alluded to by Dr. Roberts, that in a watery dilution of an albuminous urine the quantity of acid required to precipitate the albumen with heat must be very much reduced. The amount of acid must bear a certain proportion to the amount of salts present in the solution, any excess over this proportion being certain to keep the albumen in solution. Hence, after diluting with water the salt is so much reduced that the former quantity of acid proves an excess, but if we now add salt in sufficient proportion we may safely use as much acid as before. If, instead of diluting with water, we remove most of the crystalloids by dialysis, we find that we must diminish the acid in the same way to precipitate the albumen by heat; but if we again add the salts abstracted we may acidulate as freely as before. Hence, you must acidulate very cautiously in a urine of low specific gravity. A source of serious difficulty with the heat test is that the urine is sometimes naturally too acid. In this case we are advised to neutralise cautiously to a certain extent, but this is a tedious and uncertain proceeding, and how are we to know when it may be necessary? A much better plan is to neutralise the effect of acid by salt. Whether albumen is precipitated with heat or not depends altogether, indeed, on the relative proportions of the two factors, salt and acid; and of the two the former is the principal agent, and the latter the assistant—and a very treacherous one. The difficulties attending this method of testing are well illustrated by the contradictory and invariably imperfect rules given by authors.

But the most serious objection to heat and acetic acid is the very one which has been urged against picric acid,—the danger of being misled by a mucin reaction. And the very same might have been urged against nitric acid, had it been worth while; it frequently gives a pronounced mucin reaction, although I am not aware that the fact is anywhere noticed. With respect to acetic acid I have already mentioned that it is not recommended with heat, as formerly, in the later editions of Neubauer and Vogel, and Méhu *On the Urine* only advises it in large proportion with excess of salt, the urine being filtered after the addition of these two agents. It seems to be still generally supposed that the slightest haze with heat, as ascertained by comparison of the upper part of the tube with the lower, or of the heated tube with another sample of the same urine, equally acidulated, in another test tube, is conclusive of the presence of albumen. The idea, I am disposed to think, is erroneous, and the point demands the attention of those who rely on this test. I have seen a turbidity with heat when other tests gave only a mucin reaction, and Méhu states that urines containing a certain amount of mucin become muddy when boiled,* this turbidity not disappearing with acid. It is true that mucin is precipitated in the cold by acetic acid, but it requires a considerable amount of acid, and, with a small quantity of mucin, a little time to develop its maximum effect. This is much intensified by heat, so that a small amount of acid, which may have no effect in the cold, will, with a certain amount of mucin, produce a decided opacity on heating. The same happens with picric acid, a marked opacity not produced in the cold. Indeed, cold picric acid gives little mucin reaction, and even

* Page 209.

without the correcting citric acid test would seldom mislead us; the mucin precipitate, too, has a thready, peculiar appearance. I have observed these mucin reactions in solutions prepared from ox gall, as well as in the urine. I have no doubt that those who use heat and acetic acid will frequently, especially in the urine of females, find albumen where those who use picric acid find none, but only a mucin reaction. Let me mention the last case I have seen, which was a well-marked one. It was the urine of a female, the subject of phthisis, of sp. gr. 1025, very turbid from urates. It was filtered, and some of it was placed in two test tubes held side by side. It was still slightly turbid, but on heating a little both became quite transparent. One of them was then heated to boiling, and became not merely hazy, but assumed a very decided opacity. On adding an equal volume of the saturated solution of picric acid to another sample there was a scarcely perceptible opalescence, but this became, with heat, as dense as the former. On now testing with citric, picric, and chromic acids, the result was identical with all three, so that I was convinced of the absence of albumen. The three precipitates, after standing an hour or two, assumed an irregular appearance, different from albuminous ones. Further, I now acidulated another specimen of the same urine strongly with citric acid, and instantly the tube became turbid. On heating this it did not in the least diminish at any temperature; it was therefore not urates, and could be nothing but mucin. Heat, therefore, appears to stand in need of some correcting test, for this amongst other reasons.

If the foregoing observations are correct the detection of a trace of albumen in the urine is still a matter of some difficulty, especially in the presence of mucin. No test is so certain in urine as in simple water. Additional attention to the tests is still important, or we may discuss the significance of what, after all, is not albumen. I think no one should decide on the presence of a trace of albumen on the evidence of any single test. No harm, but benefit, can result from the use of the most delicate tests attainable. They in reality render testing more easy, as well as more certain; the negative result which we obtain in many cases at once puts an end to further search. The doubtful cases are but few, and after a little experience the mucin test will seldom be found necessary. The charge brought against picric acid that it reveals albuminous bodies hitherto unnoticed is groundless. As far as my own experience goes I never have found a trace of albumen by one test which I could not confirm by several others; even with a little trouble by the inferior test, nitric acid. But if the allegation were valid it would only make the use of such tests more imperative. However numerous the several forms of albumen, they all have certain characters in common by which we should first seek to recognise them in the urine, where all are pathological. If we can go farther in any case and prove that the albumen is of a special kind, so much the better. But the distinctions between various forms of albumen are not always well founded, especially in the urine, as could easily be shown if time permitted. Indeed, albumen as found in the urine never gives exactly the same reactions as the albumen of the blood, but the cause of many of its variations is still very imperfectly known.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Trichinosis.—The *Gazette Médicale de Paris* contains, in its Nos. of 29th December, 1883, and 12th January, 1884, two papers upon this subject: the first by Professor Brouardel, and the second by M. Grancher. The researches of these gentlemen have been directed to an epidemic of trichinosis, which broke out in the autumn of 1883, in Halberstadt, Germany ; and while there more particular attention has been given to the origin of the disease and its relation to imported American pork, a subject which, of late, has been exciting much interest in France, yet much that is important with regard to the symptoms and the prevention of the disease has been elucidated in the inquiry.

The origin was clearly traced to native pork, passed as sound by the public inspector, and sold in the form of sausages, which were eaten raw by the peasantry of two or three neighbouring villages. Interesting statistics are given, dealing with the vitality and reproductive power of the "trichina," as manifested by the fatal effects produced among those who partook of the flesh of the diseased animal upon the several successive days of its sale.

The mortality among those who ate upon the day immediately after the death of the animal was 33 per 100; among those who did not consume the food until the second day 16 per 100; the third day 21 per 100; fourth day 13 per 100, and among those who did not partake of the infected food until the fifth day the death-rate was reduced as low as 10 per cent.

The symptoms of the disease were for the most part of the usual description; but it was noticeable that, while the œdema of the third stage was very pronounced in the trunk and extremities, there was but little of that swelling of the face which has been described as so characteristic. In the first stage the abdominal symptoms, the diarrhœa, and the extreme prostration recalled to mind cholera; while the second stage, marked often by a considerable amount of fever and extreme lassitude, presented a similarity to typhoid. It is the third epoch of the disease, however—that of œdema and pallor—which, taken alone, presents most possibilities of mistake, leading the physician to believe that he is dealing with nephritis or cardiac disease; and Professor Brouardel has rendered its identification still more difficult by throwing doubt upon the almost universally received opinion that in trichinosis the urine contains no albumen.

The history, however, is so peculiar that, especially in presence of an epidemic, little difficulty would be experienced; it is only in isolated cases that an error of diagnosis is pardonable.

The investigation brought to light the fact that in every case where the food had been cooked before ingestion no noxious results were manifested, and this is the more interesting as in no case did the process of cooking consist of more than five minutes' immersion in boiling water. A very slight degree of cooking was thus apparently sufficient to destroy the trichina and to grant perfect immunity. Another method of prevention is described by Dr. Brouardel in the following sentence extracted from his interesting paper:—"Incidentally Dr. Philip called our attention to another mode of prevention, but one which cannot be recommended. We only mention it because it is interesting with regard to the reproduction of the trichina. He told us that a man of Emmersleben had eaten at one meal three-quarters of a pound of this raw pork, drinking along with it a bottle and a half of brandy. He presented no bad symptom. It is necessary to remark that the brandy of the district is weak, certainly not marking more than 40°."—J. Y. M.

The Treatment of Hooping-Cough.—The following extracts are made from a paper, by Dr. Sankey, which will be found in the *Birmingham Medical Review* for February last. After a few remarks on the probable causes which tend to produce hooping-cough, he insists upon the great benefit that may be obtained from the use of large and repeated doses of the carbonate of iron. This remedy is by no means a new one, but it has not been sufficiently appreciated by the profession generally. He says—"I found much difficulty in getting parents to persevere in giving the iron to their children. It was an accidental circumstance which suggested a mode of administration, and which surmounted the difficulty. Among the cases which came under my care there happened once to be the children of a baker, whose wife was a very intelligent woman, and to get her children to take the drug, she made the powders into gingerbread nuts; in this form the children took the medicine eagerly. So much success followed the remedy that the woman made 'iron cakes' and sold them. I have since recommended the dose to be concealed in chocolate cakes, and have found it equally efficacious.

"Among the cases which I have treated from time to time, during the last 40 years or more, were the children of a friend, a curate. His children had caught the disease from the rector's children. The rector's children were in the height of the disease in mid-winter, and the curate's children took the complaint in the early spring. They were cured in about three weeks; the rector's children were still coughing at Easter. The duration of these cases was at least three months; the hygienic relations of the two families were as near as possible identical.

"At first I used to be very cautious about commencing the administration of the iron on account of the apparent bronchitic symptoms, but by caution I gradually found that the remedy may be commenced at the earliest period, and that nothing but good resulted. I also tried various doses, and came to the conclusion that small doses, one grain to each year of the patient, up to five or six grains every three or four hours, were the best when made into chocolate. The quantity can be easily apportioned, and a little more or less is of no consequence."—J. A. A.

Errors in Examining the Urine for Sugar.—The following illustrates with what care and precaution every urinary examination in regard to the presence or absence of sugar ought to be made. Professor von Heusinger, in a recent session of the Aerztl. Verein in Marburg, stated that a certain individual desired to be examined in view of having his life insured. At the close of the physical examination he was requested to urinate. As he had micturated before entering the doctor's office he now could pass but a slight amount. The chemical examination gave a yellowish-green precipitate (saccharine). At the examiner's request the man returned the next morning, and the urinary test presented a negative result. It turned out after a close questioning that the individual had suffered for months with gonorrhœa, and had used injections of sulphate of zinc. He had passed water and used this injection just previous to presenting himself for the first examination. Dr. Fettien, who was then consulted, found that if a solution of sulphate of copper is added to one of sulphate of zinc and tartaric acid and caustic soda in excess, a blue fluid is formed which contains, besides the constituents of Fehling's solution, sulphate of zinc. Added to boiling urine, the zinc is precipitated as a hydrate with a greyish-green colour and the solution turns from blue to yellow. If albumen is added the same phenomena are observed, only the fluid above the precipitated zinc is reddish. —(*Berl. Klin. Wochenschrift.* 1883.) *Practitioner.* Jany. 1884.

Iodoform in Ophthalmic Therapeutics.—Vossius recommends Iodoform (*Archiv für Ophthalmologie*, xxix) in all ulcerative processes of the cornea, especially in ulcus serpens. Its use is not contra-indicated by any complication of the iris. The earlier the case is treated the more rapid and favourable will the result be. As an antiseptic in all superficial and deep accidental and artificial wounds of the conjunctiva and cornea, he strongly advises its use. He has rarely seen any evil effects, and never the amblyopia spoken of by Hirschberg, and attributed by the latter to the use of this drug.—*Medical and Surgical Reporter.* 5th January, 1884.—J. M.

The Strength of Spiritus Ætheris Nitrosi.—The strength of this preparation has been examined by Mr. Symons, and he finds that it varies exceedingly, some preparations containing as much as 4·08 per cent, and others as little as ·18. From the wholesale house which supplied him with the specimens containing 4·08 per cent other samples were obtained indirectly by purchase, and these contained respectively only ·46 and ·35 per cent. The author thinks that probably the great variability of this preparation is due to the want of proper care in condensing the distillates by means of ice.—*Pharmaceutical Journal*, p. 281, No. 694.

Excision of the Urethra for Stricture.—Dr. Haesner (*Deutsche Med. Woch.*) reports a case of stricture of many years' duration in the spongy portion of the urethra. He cut into the urethra from the scrotum both behind and in front of the stricture, and divided the latter on a sound passed into it from behind. He then excised the strictured portion of the urethra and united the ends over a Number 16 catheter (Windler's scale), which was left in for three days. After three months No. 20 could be passed with ease.—*Med. and Surg. Reporter.* 12th January, 1884.—J. M.

Cardiac Remedies.—The following table, on the action of various drugs on the cardiac apparatus, is from the second edition of Profesor G. Sée's work, *Diagnostic et Traitement des Maladies du Cœur*:—

Parts acted on.	Excitors.	Paralysers.
Cardiac muscle.	Digitalin. Iodal (small doses). Camphor. Caffeine.	Digitalin (second effect), emetine. Copper, barium, and potassium salts. Chloral (in large doses), scillain.
Cardiac motor centres.		Saponin (last effect). Iodal (in large doses).
Cardiac inhibitory centres.	Muscarine.	Atropia. Fabarine. Sparteine (large doses). Pilocarpine (secondary effect.)
Intra-cardiac plexus of inhibitory fibres of vagus.	Nicotine, } first Pilocarpine, } effect Calabar bean.	Pilocarpine (secondary effect).
Trunk of vagus.	Aconitine. Napelline.	Sparteine. Napelline (second phase).
Plexus of accelerating fibres of sympathetic.	Apomorphia.	Sparteine.
Inhibitory centres of medulla.	Digitalin.	Chloral. Croton Chloral.
Vasomotor centre.	Bromide of potassium.	Prussic acid.

Dublin Journ. Med. Science. Oct. 1883.

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MEETINGS OF SOCIETIES.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

DISCUSSION ON ALBUMINURIA, ITS PATHOLOGY AND CLINICAL SIGNIFICANCE—*Concluded.*

Session 1883-84.
Meeting IX.—*11th March*, 1884.

The President, Prof. M'Call Anderson, *in the Chair.*

On Albuminuria in the Continued Fevers, and on the Pathology of Albuminuria; Albuminuria in Persons Apparently Healthy.

By Dr. George S. Middleton.

In the course of this discussion frequent reference has been made to the occurrence of albuminuria in the specific fevers, and as this is a subject to which I at one time devoted some attention, I may be allowed briefly to record the results of my observations. These were made on all the cases of typhus and enteric fever admitted consecutively into the Maryhill, Hillhead, and Partick Fever Hospital during a period of about 15 months, and the examination of the urine in each case was made from the date of admission, and in most cases daily for several weeks.

Among 30 cases of enteric fever there was found albuminuria in 26, or 86·6 per cent; and of these, 4, or 15·4 per cent presented albumen in abundance. The presence of albumen was noted as early as the fourth day, but in most instances it was impossible to say when it first appeared, as it was observed at the first examination after admission in 20 cases, and in 15 of these on or before the 12th day. Albuminuria persisted in 7 (3 fatal) cases till the last note, but disappeared in 20 cases on or before the 30th day. All the cases in which albumen was abundant were severe, and 1 was fatal. Of the 22 in which it was slight in amount, 15 were smart cases with high fever, and 2 were fatal. Of the 4 cases in which no albumen was detected, 1 was convalescent on admission, 2 (one a child of 4 years of age) were mild cases, and 1 died of perforation on the 17th day. The average age of the 4 without albumen was 20 years, of the 26 with albumen 25 years, and of the 4 with abundant albumen 24 years. In conjunction with albumen tube-casts were found in 17 cases, in 5 of these (3 fatal) in great numbers, the casts being mainly epithelial and hyaline, though granular casts were not infrequent.

Out of 27 cases of typhus there were 24 with albuminuria, or 88·8 per cent, and in 6 of these (22·2 per cent) the albumen was abundant. Just as in the case of enteric, it was impossible in most cases to say when the albumen first appeared, but it was noted as early as the 6th day, and in 18 cases on the first examination after admission—13 of these being on or before the 9th day. Albuminuria persisted in 8 cases till the last note, 1 a fatal case, but most of them cases in which examination of the urine was not made after the 20th day. It disappeared in 22 cases on or before the 25th day. All the cases in which albumen was abundant were severe (well marked typhoid state), and 1 was fatal. Of the 18 cases in which it was slight, 16 were smart attacks, all with high fever, and many with a well marked typhoid state; 1 was fatal. Of the 3 cases in which no albumen was detected, 2 were children—the one a mild case, the other with high fever; the third (æt. 20) was a severe case, and in it a few hyaline casts were seen. The average age of the 3 without albumen was 12 years, of the 24 with albumen 25 years, and of the 6 with abundant albumen 24 years. Tube-casts were present in 19 cases, very abundant in 11 of these (1 fatal), and of the same nature as those in enteric. In one case blood was abundant (with blood casts) from 6th to 10th day. A few cases of both typhus and enteric had tube-casts without albuminuria, and in several other cases casts were seen long after albumen could not be detected.

The conclusions to be drawn from these, and other observations of which I have now no record, are:

1. That albuminuria occurs in over 85 per cent of cases of typhus and enteric, and tends to be more considerable in typhus.
2. That it occurs early in both diseases, rather later in enteric than in typhus, and persists till convalescence is thoroughly established.
3. That abundance of albumen indicates a severe case, but that severe cases (even with well marked typhoid symptoms) are not necessarily associated with a large quantity of albumen in the urine.
4. That tube-casts, hyaline, epithelial, and granular, are frequently present, and tend to be more numerous in typhus; when they are abundant the case is severe.
5. That albuminuria, so far as my experience goes, is more common in the febrile stage of typhus and enteric than in the febrile stage of scarlet fever.

In these fevers the albuminuria is associated with scanty urine, of high specific gravity. How then are the frequency of albuminuria and the scantiness of the urine to be accounted for? It is common to ascribe them simply to congestion of the kidneys, but I believe we cannot refer them entirely to physical causes, such as alterations in blood-pressure. In the continued fevers, and perhaps especially in the case of typhus, there is undoubtedly increased venous obstruction in the kidney, without increase of general arterial tension. In fact, the general arterial tension is notoriously lowered, although a very "undue quantity of excrementitious material is present in the blood," a condition said by Dr. Mahomed to produce a rise of arterial pressure. Now these are the conditions admitted by physiologists to cause a diminished secretion of urine. But, along with these alterations in pressure there is also a degeneration of the renal epithelium. We know that the cells of the tubuli uriniferi present the condition known as cloudy swelling, and many of the cells found in the urine present the same condition. I may here state that I find it difficult to agree with Drs. Coats and Greenfield in the view that renal epithelium rarely exists as such in the urine. No doubt the epithelium, when dead and desquamated, presents some points of difference from that found adherent to the walls of the tubules; still, I believe that what has so long been looked upon as epithelium in the urine is such. Besides, in cases where desquamation is going on, there is probably at the same time a proliferation of other cells, which, when found in the urine, will not present the characters of the mature cell.

It is also to be noted, that albuminuria sometimes occurs so early in both typhus and enteric that we cannot suppose the increase in the resistance to the onward flow of blood through the glomeruli to be sufficiently developed to be the sole cause of the escape of albumen; that in some cases the albumen in the urine does not increase in amount, while the obstruction to the flow of blood through the kidney is increasing; and that in other cases the albuminuria persists after the causes of stagnation of the circulation in the kidney have disappeared, as evidenced by an increase in the flow of urine. I believe, therefore, that while the condition of the circulation in the kidney is probably sufficient to account for the scantiness of the urine, we must regard interference with the nutrition and functional activity of the renal epithelium, by the fever poison and the effete products circulating in the blood, as an essential element in the causation of the albuminuria.

Whether it is the epithelium covering the glomeruli, or that lining the tubules that is interfered with, it is impossible to say with certainty. Two theories of albuminuria have been presented to us in the course of this discussion, based on different views as to the normal function of the glomerular and of the tubular epithelium, and either of them would readily explain the occurrence of albuminuria in fever. While I cannot add any original observations in support of either the one view or the other, I venture to submit that one of these theories—viz., Dr. Newman's, has been apparently misunderstood, if we are to judge from the criticisms passed on it by Drs. Coats and Greenfield. Neither of these speakers seemed to recognise the important part played by the lymphatics of the kidney in that theory, nor the fact that the theory is not a purely physical one, a selective power of absorbing albumen being attributed to the epithelium of the tubules.

For the objection raised by Dr. Coats to the application of Dr. Newman's theory to the case of acute Bright's disease is, that the epithelium is said to be so paralysed as to be unable to absorb the albumen while it absorbs a large amount of water. Now Dr. Newman, as I understand him, did not say that the epithelium absorbed the water: in his view it is the "condition of the lymphatics and the proximity of the blood-vessels to the uriniferous tubules" that determine the absorption of water, and it is the epithelial cells of the tubules which have the power of selecting albumen from its solution in these tubules—a power you will observe not mechanical, but vital. In the case of acute Bright's disease, therefore, the explanation of the scantiness of the urine is that the mechanical process of absorption is not interfered with, while the presence of albumen is to be accounted for by the vital activity of the epithelial cells of the tubules being in abeyance.

Again, in regard to the case of chronic Bright's disease, Dr. Coats says, "We are asked to believe that while the water is left alone the albumen is absorbed, for in many of these cases of extremely contracted kidney the albumen in the urine is a mere trace." Now this theory does not seek to maintain that the blood plasma escapes as such into the terminal dilatations of the uriniferous tubules, but that only a very diluted blood serum so escapes. In other words, it is not contended that a fluid, rich in albumen, finds its way into the uriniferous tubules in health. In the contracted kidney, from the thickening of the walls of the glomeruli, the quantity of albumen transuded will be even less in amount than in the normal condition, and there will be enough of tubular epithelium left to carry on re-absorption of albumen to some extent, for it is admitted by Professor Greenfield that atrophy of the tubules is secondary to atrophy of their glomeruli.

In support of the view that the walls of blood-vessels everywhere exercise something of a selective power on the constituents of the blood which they allow to transude, Dr. Coats instances the cerebro-spinal, the peritoneal, and the synovial fluids. Now, these are all fluids containing more or less albumen; and all that is contended for in the case of the kidney is, that the urine as secreted by the glomeruli is no exception to the general rule that transudations from the blood contain albumen. The amount of albumen that transudes through the glomeruli will of course vary, depending mainly on the quality of the blood, the condition of the walls of the vessels in the Malpighian bodies, and on the tension of the blood in those vessels.

When we consider the numerous convolutions in the tubules, and their constriction just as they are about to discharge into the collecting tubes, it appears as if this were an arrangement designed to delay the fluid in these tubules, so as to allow of some re-absorption. The form of the epithelium lining the tubules seems also to favour this view. For over the glomeruli we have a fine squamous epithelium, but in the tubules the epithelium is comparatively thick and globular; and while all are agreed as to the substantial nature of the lining of the tubules, there is a difference of opinion as to the existence of a "layer of basement membrane and epithelium" covering the glomeruli; and even Dr. Coats quotes his description of it from Heidenhain, as if he were unable to describe it from personal observation. Dr. Coats attaches great importance to it, however, and compares the glomeruli so covered to the alveoli of the lung; but this is a faulty analogy, as the conditions of circulation in the vessels of the glomeruli are very different from those in the capillaries of the alveoli. For in the normal condition there is no resistance offered to the escape of blood from the pulmonary capillaries corresponding to that offered by the efferent vessel and the true capillaries of the kidney. But when an obstruction to the flow of blood through the lung does arise, as from mitral disease, then we find that there is produced an œdematous state of the lung. In other words, when the conditions of circulation are made similar to those that exist normally in the kidney, then a transudation of an albuminous fluid takes place. If, however, the obstruction offered by the mitral valve be compensated by increased action of the right ventricle, then there may be no œdema. There will be increased tension in the pulmonary circulation, and probably some fluid will transude, but not more than can be readily absorbed by the lymphatics of the lung. When, however, the compensation fails, then œdema occurs. This goes to show that increased afferent pressure is not sufficient to cause a great transudation; and, by analogy, we may infer that increased arterial tension is not enough to give rise to albuminuria.

Dr. Greenfield prefers to compare the glomerular tuft to the salivary and gastric glands; but the conditions of circulation there also are different from those in the glomeruli. I would rather compare the epithelium lining the uriniferous tubules to that covering the villi of the intestine—the characters of the two being somewhat similar, as is also their relation to the vascular system. If this analogy holds good, then, so far as it goes, it is in favour of the view of the function of the epithelium of the tubules, which seems to me the most feasible one; for it appears to me more likely that the very delicate covering of the glomeruli allows albumen to transude, and that the thick epithelium of the tubules has a selective power in absorbing albumen, than that the former has the power of preventing the escape of albumen, while the latter is occupied solely in absorbing fluid, unless we suppose that it is also occupied in the separation of urates, uric acid, &c. In this connection I would quote a passage from Dr. Greenfield's remarks which, with the alteration of a single word, seems to me to apply very well to the theory I am supporting—"If we find that a large number of altered conditions, many of them of extremely slight character, cause the presence of albumen in the urine, is it not in favour of the view that there is a mechanism, whatever may be its nature, which is very readily deranged, but which normally prevents the *escape* of albumen. If fevers of all sorts, and blood poisonings, various vegetable and mineral poisons, derangements of circulation due to nerve lesion, alterations of pressure which we may call mechanical, produce albuminuria, must there not be some easily disturbed mechanism by which the *escape* is ordinarily impeded?"

Dr. Greenfield asks why, in the case of hysterical polyuria and in diabetes, the urine contains no albumen, if albumen escapes normally from the glomeruli? According to this view, it is simply because in these diseases the renal epithelium is not involved, and therefore absorbs the albumen, even though more in amount than usual, for we know that most of the tissues and organs of our body have a certain reserve power capable of being called into play in emergency. And it is to be noted, as admitted by Dr. Greenfield, that, when in diabetes the epithelium of the tubules does become involved, then we have albuminuria.

It was said by Dr. Mahomed that to suppose albumen to escape and to be again absorbed was attributing to nature a clumsy method of performing her work. But he appears to have overlooked the fact that it is generally admitted that more fluid is exuded from the glomeruli than reaches the pelvis of the kidney; in other words, water is transuded and then re-absorbed, a clumsy contrivance, which I believe is admitted by himself.

It seems to me, then, that the arguments brought forward against Dr. Newman's theory, and some of which only I have adverted to, have failed to disprove it; and as it explains the occurrence of albuminuria very simply, I would adopt it till we get more light on the matter.

Its application to the case of the specific fevers is very easy. In these, as we have seen, there is a structural change in the epithelium of the tubules evidenced by cloudy swelling. But, in addition, there is probably interference with its functional activity; for we know that in the pyrexial condition there is interference, more or less, with all the functions of the body. Hence, the albumen, which probably transudes in somewhat larger amount than normal through the glomeruli, will in part fail to be re-absorbed, owing to this impairment of the functional activity of the epithelium of the tubules.

It affords also an exceedingly simple explanation of the occurrence of albuminuria in persons apparently healthy, which we must take to be an established fact, though we do not therefore call it a physiological albuminuria. For if a condition of the epithelium of the tubules of the kidney were induced by any cause, such as indiscretion in diet, &c., similar to the changes that we know occur in the epithelium of the stomach and intestines, say in dyspepsia, it is evident that small quantities of albumen might thus escape re-absorption from impairment of the function of small portions of the kidney tissue. If so, then, either recovery might take place, or the condition might remain permanent but local, or, more probably, it would extend and involve other sections of the kidney. As this is an important subject, especially from a life assurance point of view, and one as to which our knowledge is as yet deficient, I may be allowed to quote somewhat in detail from a valuable report presented to the Board of Directors of the United States Life Assurance Company by one of their medical examiners, Dr. Munn. He states that "eleven per cent of those presenting themselves for assurance had albuminuria, attended with no discomfort or unpleasant symptoms, and with no discoverable cause for its existence." The total number of such cases was, in 1878, 24; in 1879, 19; in 1880, 26; in all, 69.

Of the 43 met with in 1878 and 1879, he was able in 1880 to report the condition of 25, with the following results:

"Three of the number have died. The weight of the body was found unchanged in 7 cases. It was diminished in 2, and increased in 20 of the number. The pulse was unchanged in 7, diminished in 6, and increased in 19 cases. The change in this respect, however, has been so slight that little importance can be attached to these data. Albumen has disappeared in 3, diminished in 3, remained unchanged in 13, and increased in 11 cases. Casts were found in 8 cases, hyaline in 7, and granular in 1. With two exceptions, each individual declared himself to be in good health, and said that his condition remained unchanged, though in after conversation it appeared that slight discomforts were manifested in some cases. No change in appearance was noticeable save in 3 cases. In those cases which presented casts, the weight remained unchanged in 2, and increased in 6, though not to any great extent. From the fact that albumen and casts may temporarily disappear from the urine, and then re-appear, we can neither say positively that casts have been developed during the past year, nor that the albuminuria has permanently ceased in the 3 cases in which it is not found at the present time. . . . In view of the fact that 4 of the number have died (1 among those first seen in 1880), and that the general appearance of the majority of those who have been under observation for more than one year is gradually deteriorating, I am led to believe that albuminuria should be regarded as of grave significance. In some cases, however, it may be of slight importance, and further research may enable us to discriminate between them." With these conclusions most of us will agree.

Albuminuria considered as chiefly caused by Deteriorated Vitality, or Degenerated Structure, of the Renal Vessels.

By JOHN LINDSAY STEVEN, M.B.

I intended, Mr. President, at the outset of my remarks, to refer to certain objections which appear to me to be raised against the term "physiological albuminuria," but as these have already been considered in great detail by Professors Gairdner, Greenfield, and others, I need not further allude to the matter. I would only seek to add, with Professor Greenfield, that I "cannot admit a physiological—that is, a healthy albuminuria." But—coming now to the important question of the causation of albuminuria—I think that the large class of cases described as physiological may be regarded as throwing much light upon this somewhat difficult and interesting problem. In such cases increase of blood pressure, whether proximal or distal, as an exciting cause, may be set aside: it can play no part in them, for the conditions which give rise to it—viz., circulatory obstruction, either within or without the kidney—are absent. What, then, is the cause of the albuminuria in such? In endeavouring to answer this question, I remark, in the first place, that I believe the normal function of the kidney is to separate water, urea, and the other salts met with in the urine, from the blood, and to prevent the passage of albumen through the walls of its blood-vessels. Whenever, therefore, albumen appears in the urine some alteration of the function—very often also of the structure—of the organ has occurred, and this alteration I believe to be some deterioration in the vitality of the vessels of the kidney, which prevents their retaining albumen, and which may or may not be a persistent condition.

Now, apart from the kidney altogether, we know that diminished vitality of the vessel wall often allows not only albuminous fluid in excessive quantity, but frequently also the blood corpuscles themselves to leave the capillaries. For example, an active hyperæmia, accompanied by great dilatation of the vessels and great increase in the speed of the circulating blood does not of itself cause œdema; but, if an irritant be applied, and thus the normal vitality be impaired (the initial stage of inflammation being induced), an œdema readily takes place—an excessive quantity of albuminous fluid which the lymphatic vessels cannot dispose of passes out into the interstitial spaces of the tissues. Of great importance in this regard are Cohnheim's experiments on the vessels in the ear of the rabbit. But on the other hand, to pass to venous obstruction, we know that passive or venous hyperæmia is very often not at first accompanied by œdema, even although the increase of blood pressure may be considerable; but if the stagnation be long enough continued to diminish (by prolonged contact of venous blood) the vitality of the vessel wall, then œdema results—in this case the concentration of the exuded albuminous fluid being less than in the former, because the deterioration of vitality is less, and more gradually induced. Again, in cases of anæmia we know that œdema of the ankles is a very frequent concomitant—here, from the nature of the case, there can be no increase of blood pressure, but the blood is defective in nutrient qualities, and so the retaining power of the capillaries becomes much lessened.

Turning now to the case of the kidney itself, we are not without evidence that a simple increase of blood pressure *per se* is not sufficient to give rise to the presence of albumen in the urine. I do not wish to insist upon the experimental evidence based upon the endeavours to elevate the blood pressure by ligature of the abdominal aorta, as it has been urged by Dr. Mahomed that these experiments have been unsatisfactory; but still it may be mentioned that Frerichs and Munk are of opinion (based on experiments of this kind) that increase of pressure alone is not sufficient. But leaving experimental research aside and coming to pathological conditions, we shall not fail to find corroborative evidence. In diabetes mellitus there must, whatever else there is in its pathology, be an enormous increase in the amount of blood passing through the vessels of the kidney, and probably also an increase of its pressure, and yet how rarely do we find albuminuria associated with this disease when uncomplicated. In this regard the following figures may perhaps be of some interest. On referring to my notes I find that I have been asked at different intervals to examine (with special reference to the quantity of sugar present) the urine of 11 patients suffering from typical diabetes mellitus, and that the actual number of examinations made was 53. In all these examinations a specimen of the 24 hours' urine was employed, and a careful note was made of the presence or absence of albumen. I was only able to detect albumen in 8 (a very small fraction over 15 per cent) out of the 53 specimens examined. In the urine of one of the patients, examined 4 times during the residence in hospital, albumen was found on every occasion; in that of the remaining 4 patients it was only found once in each series of examinations—viz., in the first once in 8 examinations, in the second once in 6, in the third once in 5, and in the fourth once in 6. It will thus be seen that in the case of only 1 out of the 11 patients was albuminuria a constant feature, and that in the remaining 4 it was very occasional. These results are important when we consider that in order to the tremendous polyuria we must have a large excess of blood, in all probability under circumstances of increased pressure, passing through the glomerules; and here, too, we have a strong argument in favour of the view that increase of blood pressure *per se* is insufficient to set up albuminuria. Professor Greenfield points out that when albuminuria co-exists with diabetes mellitus it is to be regarded as an indication of renal degeneration or of secondary complication. Of importance, too, in this regard are Rokitansky's observations on the appearance of the kidneys in 20 cases of diabetes after death. The changes were not uniform; the organs usually presenting indications of hyperæmia, being enlarged, dark red, and full of blood. "Occasionally there were signs of more extensive mischief, the substance of the kidney as well as its vessels and epithelium being involved, and the organ harder than natural. Often fatty changes occur." * The former changes are indicative of engorgement, and may be present without albuminuria; the latter are what we would expect to find in the later stages, when albumen is often found in the urine. In this disease I can only account for the albuminuria, when it is present, on the supposition,

* *A Dictionary of Medicine.* R. Quain, M.D., London, 1882, p. 348.

not that blood pressure is increased, but that the vitality of the renal vessels, in common with the other tissues of the body, has been lowered.

Again, the tremendous polyuria of diabetes insipidus can only be explained on the supposition that an enormous hyperæmia of the kidneys, nervous in origin no doubt, has taken place, and yet albuminuria is almost never found in this disease—a circumstance, I think, strongly in favour of the view I adopt.

In the albuminuria so frequently met with in the course of febrile affections, it is not likely that increase of pressure is the cause. It is much more likely, as is indicated by the suggestive remarks of Dr. Finlayson, that it is toxic in origin; and I am decidedly of opinion that poisons are injurious, chiefly by deteriorating the vitality of the tissues. I cannot see how poisons circulating in the blood will tend to raise the pressure in the kidneys more than in any other part of the body; but all analogy points to their exercising deleterious effects on the tissues otherwise. Thus, the tubercular poison invariably excites caseous necrosis, and the pyæmic virus calls forth destructive inflammatory changes; and there is nothing unjustifiable in our supposing that they may exercise similar effects upon the kidneys.

The fact, too, that nervous disturbance, such as has been specially referred to by Dr. Mortimer Granville, may cause albuminuria need not necessarily be taken as proving that increase of blood pressure alone is the explanation, because we know that the nerve centres also possess trophic properties, and that many of their lesions are accompanied by the most pronounced atrophy and wasting of parts—*e. g.*, infantile paralysis. The same conditions may hold good with regard to the nervous supply of the kidneys.

For these reasons, therefore, I am very strongly of opinion that increase of blood pressure of itself has comparatively little to do with the causation of albuminuria, while deterioration of tissue structure has a great deal.

I would now crave your attention for a little, while I endeavour very briefly to apply these views more particularly to diseases of the kidneys themselves. With the very able remarks of Professor Greenfield on albuminuria in heart disease I almost entirely agree; but I would be inclined to attach more weight than he does to degeneration of structure, and not quite so much perhaps to increased pressure. It is quite unnecessary for me to go into detail in showing how the theories I have mentioned account for the albuminuria of organic diseases of the kidney, as this subject has already been fully discussed by Drs. Coats and Greenfield. But, as an argument from analogy, I would point out that the inflammatory process is one of the most common causes of alteration in the vitality of the blood-vessels, or of degeneration of their structure—and given this alteration abnormal transudations will occur. It is of interest to compare the exudation in an acute and chronic inflammation —take for example the peritoneum. In acute peritonitis the fluid exudation is scanty, highly albuminous, readily coagulating, and matting together the loops of intestine by bands of fibrine; in chronic or in tubercular peritonitis it is much more abundant, not nearly so concentrated, and fibrine, if present, is only so in shreds. To me there seems little difficulty in accepting the view that this is precisely what takes place in the acute and chronic forms of Bright's disease. And, on the part of those who maintain that increase of blood pressure is a main cause of albuminuria, it is no sufficient answer to this to say that the scanty albumen of chronic Bright's disease is due to the fact that the epithelium is healthy, and re-absorbs the greater part of it.

I confess, too, that it was not without surprise that I heard Professor Hamilton (the same idea was also indicated in Dr. Newman's remarks) make the statement that he would not be surprised if a very large quantity of water was secreted by the glomerulus in acute Bright's disease, but that it never got to the bladder. All analogy teaches us that acute inflammation of any part arrests its function, and why should the kidney be an exception to this rule? Again, if the glomeruli were passing an increased quantity of water to be again absorbed by the lymphatics, one would expect that there would be evidences to the naked eye of an œdema, localised to the kidney, its capsule, and its immediate neighbourhood. I have never had my attention drawn to a condition of this kind in the dead-house, nor do I, in the course of my own *post mortem* experience, ever recollect to have seen it. And yet we know, as has been shown by Cohnheim and Lichtheim's artificial plethora experiments, that a large excess of watery fluid, passing through the kidneys in a state of health, may cause an œdema of these organs.

In the course of his remarks Dr. M'Gregor-Robertson said, "as regards the part played by the glomerules, it is universally admitted to be a process of filtration;" and he expressed surprise that Dr. Coats should be exceptional in not accepting this view. Such a statement would appear to indicate that

Dr. Robertson was not acquainted with, or at least had not read, one of the most masterly expositions of urinary physiology of recent years—I refer to the able monograph of Heidenhain in Hermann's Physiology. There I find it stated that a process far more intricate than simple filtration is going on, and that the epithelial cells of the Malpighian tuft actually, as Dr. Coats implied, carry on a secreting function. I may quote one of many sentences in this regard:—"So far as I see, the conclusion is unavoidable that the separation of water in the kidney depends upon an *intense activity of the cells of the glomerular vessels, of which the amount is fixed by the quantity of blood passing through in a given time.*" * At another part of the article a whole series of reasons are urged against the filtration hypothesis. In a still more recent work on the subject published conjointly by an observer whose name has been frequently quoted in this discussion—Professor Leube of Erlangen—I find Heidenhain's views are closely followed, and there the filtration process is regarded as old.† Similar views are held by one of our greatest modern pathologists, Cohnheim. It will thus be seen that Dr. Coats is not exceptional in the position he takes up, and that the filtration theory is very far from being universally admitted. Apart from experimental physiology altogether, however, there are certain phenomena which seem to me to render it perhaps doubtful whether a simple physical transudation can occur in any of the living capillaries. Take any transudation you like and you will find that in chemical composition there is often more than a mere quantitative difference between the fluid on the filter and the filtrate. Hydrocele liquid is rich in fibrinogen, whereas cerebro-spinal fluid usually contains no fibrinogen, but little albumen, and often a distinct trace of sugar.‡ Now, how are we to explain these differences? Evidently by the fact that the vessels have some selective power, and not by a mere process of filtration.

It is extremely difficult to form a definite opinion upon a single experiment, and certainly the investigation by means of atropine, as it was stated by Dr. Robertson, can quite justifiably be interpreted either way. It may have been the glomerular and not the tubular epithelium that was interfered with.

With regard to the theory, too, that in the normal condition

* *Handbuch der Physiologie*, edited by Hermann. Leipzig, 1880. Vol. V, p. 331.

† *Die Lehre vom Harn.* Berlin, 1882. Page 308.

‡ *Physiological Chemistry*, by A. Gamgee, M.D. London: Macmillan & Co., 1880. Vol. I, pp. 235, 236.

albumen passes through the glomerular vessels, I wish only to say that this seems to me to be a somewhat round-about way of explaining a natural phenomenon like the secretion of urine. It is a process in which we cannot see any very definite object to be gained. Why should the vessels of the kidney pass serum-albumen if the epithelium and lymphatics are only to have the trouble of re-absorbing it? If the vessels were dead structures, and thus—which they are not—subject to the purely physical laws which regulate filtration in dead membranes, then I would not have the same difficulty in accepting it. In the course of my paper I have indicated my belief that even in the simple systemic capillaries there is more than a mere filtration involved; but in the glomerular vessels we have in addition a distinct epithelial layer, which covers not only the tuft but also the individual lobules of the tuft. Surely this layer is not present without some specific function in view, and in all probability this function is more or less of a secreting or selective nature.

I would, therefore, Mr. Chairman, in conclusion, sum up my remarks by making the following statements:—(1.) That in a typically healthy kidney albumen never passes through the walls of the glomerular vessels. (2.) That when albumen is found in the urine the fact of its presence is to be explained by some deterioration of the vitality or degeneration of the structure of the glomerular vessels, and that the nature of this change may be very various, *e. g.*, inflammatory, acute, or chronic—amyloid—the result of poisons—simple diminution of tone, &c. (3.) That increase of blood pressure, whether arterial or venous, of itself seldom, if ever, causes albuminuria, unless associated with the degenerative changes mentioned above.

Physiology of the Kidney in Relation to Albuminuria.

By PROFESSOR CLELAND.

Having been asked to contribute to the present discussion on albuminuria, it was with much regret that I found that I was unable to be present to-night; but this is of the less consequence seeing that latterly my time has not been devoted to purely pathological questions, and as, by committing what I have to say to writing, and entrusting it to Dr. Newman, I at once secure brevity and give my remarks the advantage of his able and friendly comment.

To my mind it is plain that in the secretion of urine there are two processes, one purely mechanical and the other fundamentally vital. There is no evidence that simple squamous epithelium ever takes on itself a secreting function, and therefore the anatomical evidence is in favour of the action of the Malpighian corpuscles being an action of pure filtration. But we have to recollect that while solid urine is passed by birds, reptiles, and fishes, Malpighian corpuscles are not peculiar to the mammals. In fact, glomeruli are very considerably developed in birds. If in birds and serpents the glomeruli allow filtration of fluid, however, then the watery substance filtered through them is nearly all re-absorbed by the tubules, and it becomes obvious that the use of the Malpighian corpuscles in these animals is not the removal of water from the blood. But the result of their filtering action must be to place less watery blood on the deep side of the epithelium of the tubules, and a watery solution within the lumen of the tubules. The question not determined is to what extent the epithelium operates on each of the two fluids.

Dr. Greenfield has correctly drawn attention to the circumstance that the afferent arteries are highly muscular. while the efferent vessels of the glomeruli are "mainly elastic." I freely confess, however, that I never saw two efferent vessels coming from one glomerulus, and I have no doubt that during life the opening of exit of the blood from the glomerulus is much smaller than the aperture of entrance. One gets injections in which, while the capillary network has been well distended, the afferent vessels are not in a state of distension, and I cannot understand how it can be doubted that the afferent vessel is larger than the efferent.

But even Dr. Greenfield himself seems to doubt his anatomical position, for he supports his objection to the idea that the circulation in the glomerulus is normally under increased pressure by saying that "in the normal condition of circulation the volume of blood entering the glomerulus must equal that issuing, and the velocity will be, speaking roughly, inversely to the area of section." Well, if we grant that there is no filtration, the issue by the efferent vessel will of course be equal to the quantity entering by the afferent; but if that be accomplished by increased velocity, what causes the increased velocity? Why, the energy exists as pressure in the glomerulus, which becomes converted into velocity in the narrow efferent vessel.

I do not know that we are in a position to decide whether ordinary serum-albumen usually passes through the glomerulus. The experiments quoted by Dr. Mahomed, in which a normal kidney was cut from the living animal and plunged in boiling water, do not go for much, for no one supposes that the albumen passes through with the ease with which it is assumed that the salts must, and a weak solution of albumen would certainly escape detection by so rough a test. But the argument is this, that the filtration from the glomeruli being purely mechanical, it follows that whether there be any small escape of albumen normally or not, the amount of albuminous filtration must vary with favouring mechanical and chemical causes. By chemical causes I mean alteration in the character of the albumen in the blood; while the mechanical causes which occur to me are, besides the pressure of the blood, the thickness of the coats of the glomerulus, and the amount of gorgement of the neighbouring tissues. This last will exert counterpressure resisting the expansion of the glomerulus.

I have asserted that the action of the epithelial corpuscles of the tubes is vital; but, like all other vital action, it is subject to mechanical conditions. Dr. Coats has objected to Dr. Newman's allegation, that in acute parenchymatous nephritis the albuminous character of the scanty urine is owing to paralysis of the epithelial corpuscles, that such paralysis does not hinder them from taking up water from the contents of the tubules. I see no difficulty in explaining that. Secreting corpuscles have a state of functional activity and a state of nutrient activity; and as would appear to be the case in muscle also, the one condition is exalted when the other is in abeyance. It seems very rational to suppose that, in a state of exalted or somewhat perverted nutrition, owing to the changes in the blood supply, they should do their work imperfectly or not at all. But the engorgement of the organ presses on the lumen of the tube, and according to the general law of absorption, apparently a mechanical law, favours the taking up of its watery contents. On the other hand, in chronic interstitial nephritis, while the epithelial elements are uninvolved, they are able to do their duty; but the changes in the interstitial tissue interferes with the circulation so as to exalt the pressure in the glomeruli, and it may be that the swollen state of the tissues of the glomeruli is more favourable to the filtration of water than albumen.

Returning to the subject of normal function. It is not easy to see what the meaning of the loops of Henle is, if there be no function of re-absorption in the previous part of the tubules. On the other hand, it is not easy to see what is the use of the enormous collection of straight and convoluted tubes subsequent to these loops, if they have not some secreting function; and on this account I have never felt sure that there is not some truth in the notion which was once broached by Henle, that urea and uric acid are secreted by the latter.

Albuminuria in its Clinical Aspects.

By Dr PERRY.

Mr. President and Gentlemen,—At this advanced stage of the discussion I shall not enter into the question as to whether or not there is such a condition as a healthy or physiological albuminuria, but I shall only remark that some of the cases brought forward by Dr. Roberts to prove its existence were rather of a doubtful character, especially those so-called healthy patients who came for advice to the Birmingham General Hospital.

From a clinical point of view, upon which aspect alone I think I would be justified in offering a few remarks, I would say, in the first place, that Dr. Finlayson in particular, and some of the other speakers, have anticipated a great deal of what I felt inclined to say on the subject.

I agree with Dr. Finlayson that "albuminuria seems to be oftenest due to the influence of poisons operating on the system," but I do not quite agree with him that in the chronic cases it is ever due "to the presence of grave *constitutional* disturbance," because in such cases I maintain that the poisons have produced also grave *organic* mischief. If the organic changes are included under the term "constitutional disturbances," of course we agree on that point also.

I admit, of course, the existence of albuminuria from inflammation of the kidneys—also, that there may be a few cases of purely nervous origin—and that whatever produces congestion of the kidneys tends to the production of albuminuria. This discussion has prominently pointed out the fact, which was already well known—viz., that there are also a great many cases of what may be termed temporary albuminuria, either from specific organisms, or from specific or other poisons circulating in the blood. The albuminuria produced under any of those circumstances to which I have briefly alluded is a perfectly curable disease, and not such a dreadfully incurable malady as we were taught some years ago to consider it.

I think, however, that too little importance has been assigned in this discussion to the products of digestion as a fertile cause of the chronic albuminuria.

Like other hospital physicians, I have seen a great many cases of albuminuria, but in hospital practice, if we except the acute parenchymatous nephritis, we rarely have an opportunity of studying the earlier stages of the amyloid and interstitial forms of kidney disease ; and from the great proportion of our cases coming into hospital at a rather advanced stage, we are perhaps a little apt to have our impressions and ideas influenced by the more marked and severe symptoms present in the advanced stage.

I am not an advocate for the subdivision of Bright's disease into distinctly separate varieties, seeing that we have a great admixture of the same pathological conditions in the different types of Bright's disease, but for clinical purposes I find it is useful to divide the chronic cases according to their probable cause.

Now, leaving out of the question meantime those cases of inflammatory origin, those of a temporary nature, and such as are caused by the presence in the blood of a few well known toxic agents, whether introduced from without, or formed during the course of such recognised diseases as rheumatism, jaundice, diabetes, the exanthemata, &c., I am inclined to divide the remainder in a general way into two main divisions, according to the antecedent history of the individual affected.

The first class would include the cases of amyloid degeneration, which have for their antecedent some purulent discharge, from whatever source that may have arisen—the most decided in its character being a suppuration in connection with diseased bone. So closely allied to this is the albuminuria following scrofula, phthisis, and syphilis, that I place them all in the same division.

There is one special observation I would wish to make respecting the amyloid disease, and I should like to know if the same circumstance has been observed by other practitioners. It is this, that since the introduction of the antiseptic treatment of surgical affections I have noticed this particular type of albuminuria becoming rarer and rarer every year, in fact, it is almost never met with in my practice in the pure and unmixed form in which I used to see it before the adoption of antiseptic surgery. It may be objected that we still have as many cases of amyloid kidney as a consequence of phthisis, syphilis, and cancer, &c. The degeneration of the kidney which follows those affections is, in my experience, much more of a mixed nature, tending frequently to fatty kidney. Dr. Coats, in speaking of amyloid kidney, remarked that the quantity of albumen present in the urine is usually large. In purely amyloid disease I have usually found the quantity of albumen small, except when symptoms of acute nephritis supervene, which in such cases is a very frequent occurrence.

The second class would include chronic diseases of the kidneys, brought on by the mode of living of the individuals, and especially by the abuse of various alcoholic stimulants, and by other irregularities of living.

It is a recognised fact that the earliest symptoms of chronic albuminuria are very frequently preceded by impaired digestion and gastric disturbances. In opposition to Dr. Hamilton, I agree with Dr. Johnson in his theory that a poison in the blood causes hypertrophy of the arterial coats, or the cardio-vascular changes. Owing to the defective digestion, the blood is not furnished with the proper and healthy material for the nutrition of the tissues, or it is loaded with irritants and stimulants from the digestion of superfluous or improper food and alcoholic fluids. The liver and the kidneys are the first organs to suffer, and we have diseased processes set up either in the coats of the vessels, or in the surrounding tissues, or perhaps in both, permitting the escape of albumen into the tubules of the kidneys; or, as Dr. Johnson says, "renal degeneration is a consequence of the long continued elimination of products of faulty digestion through the kidneys."

I am not aware what the experience of other practitioners may be on this point, but I may state that I have very rarely met with a patient who has been a total abstainer during his whole life, and who was suffering from interstitial nephritis. Of course, total abstainers are as susceptible as other people to albuminuria from acute parenchymatous nephritis, produced by cold or specific poison, such as that of scarlatina, diphtheria, or typhus, or as a consequence of some antecedent purulent discharge. We very frequently meet with patients suffering from albuminuria who tell you that they have been total abstainers, or nearly so, for perhaps five or six years, or more. On inquiring into their history prior to that time you will generally find that they had used stimulants too freely, and that in consequence of ill health, or for some other reason, they had abandoned their previous habits. Now, the foundation of the chronic kidney affection was laid before they changed their habits, and we know how seldom such chronic affections disappear when once formed. And here again I would remark that too great prominence has generally been given to the injurious effects of alcohol, and too little account taken of the excrementitious and other poisonous products of imperfect digestion of over much or improper food, but when we have the combination of the two, although the former, viz., the alcohol, may not be taken to any great excess, we have the two most active factors in the production of cardio-vascular and renal degenerations. I think there is yet a wide field for original research in this line, and I venture to say in conclusion, that were more respect paid to that much abused organ, the stomach, fewer cases of chronic albuminuria would occur.

Testing for Albumen—Minimal Albuminuria—Quantitative Albumen.

By GEO. OLIVER, M.D.

Mr. President and Gentlemen,—I fear that the few remarks I am about to submit to you will form but a trifling contribution to this important discussion; I will, therefore, present them as briefly as I can.

The fundamental matter of the best modes of testing in albuminuria has been so fully and ably discussed by previous speakers (Drs. Roberts, Mahomed, and Kirk) that any further statements pertaining to it must appear to you superfluous. I will, therefore, confine my remarks to a few data which appear to me to qualify or to supplement what has been advanced.

1. *The preliminary search for a proteid in the urine.*—Inasmuch as we do not know the clinical significance of certain recognised forms of albuminous substance, which, besides ordinary serum albumen, may appear in the urine—such as peptone, hemi-albumose, syntonin, &c.—it will surely conduce to a more comprehensive and definite study

of albuminuria to select, in the first instance, a test that will net, as it were, every variety or phase of proteid matter; and then, if it be thought desirable to differentiate between them, to apply other tests for this purpose. Of all the tests now in use heat does not fulfil the required conditions of the pioneer test, for when albumen is present in an alkaline or too acid urine, a tedious and uncertain process of rectification of the chemical reaction must be resorted to before it can be applied, and certain phases or forms of albuminous matter do not respond to it at all. For example, I have here a normally acid urine which, either after the addition of one drop of acetic acid—as advised by Dr. Roberts—or without acidification, on boiling provides a slight opacity, and, therefore, contains but a very small quantity of albumen; but, on using the mercuric, picric, or tungstate test, you observe a very copious precipitate, which, dissolving on boiling and returning as a diffused opacity as the urine cools, represents, 5 per cent of proteid matter in the form of peptone. I grant you that such a urine—so highly, and, in proportion to the albumen, so largely charged with peptone—is but rarely met with; but one must bear in mind that once having been encountered, it may turn up again. But why study peptonuria along with albuminuria? Chiefly because we do not, as yet, know the clinical relationship of peptone to ordinary albumen. Why, for example, should the two be found so frequently in the same urine, and may not peptone, sometimes in varying degrees, replace albumen and have the like pathological significance? Or, again, may not the presence of this diffusible form of albumen in the urine be a significant clue to the condition of the proteids in the blood of particular cases of albuminuria?

But, should it turn out that peptone is only casually, and not essentially, connected with albuminuria, it will surely, in the meantime, be to our advantage to use such tests as will show when we encounter albumen or peptone alone or associated.

In testing for the albuminous matter—which in the urine is truly protean in its features—I am persuaded that it is unsafe to rely on any one test, and that, though heat as a preliminary test is less trustworthy than others, it should never be dispensed with when we proceed to determine the condition of the proteid body.

2. *Minimal albuminuria.*—By this term I will venture to group those small proportions of albumen appearing in the urine that are either only just discoverable by heat or by

nitric acid, or are only detectable by more searching reagents. This group is interesting to us in its chemical and clinical aspects.

From the purely chemical standpoint, it has been alleged that a difficulty in detecting with certainty these small quantities of albumen in the urine is presented by mucin—an albuminoid body which reacts with the tests requiring an organic acid, and with picric acid. It is true that mucin does now and then appear in the urine in sufficient quantity to be recognised by acetic or citric acid, but in my experience this is by no means a common observation. But is pure mucin ever met with in the urine apart from albumen or a proteid? "Mucus contains, besides mucin, which is its chief organic constituent, small quantities of proteid substances." (Gamgee—*Text-book of the Physiological Chemistry of the Animal Body*, p. 257.) But apart from this general fact, I would refer to corroborative evidence afforded by the microscope in over 100 cases in which the mercuric, tungstate, and ferrocyanic tests, and in which picric acid afforded evidence of minimal proportions of albumen: for every one had some organic elements (such as leucocytes—in almost all—blood discs, spermatozoa), themselves rich in proteid matter, and derived from a highly albuminous fluid; in 4 cases there were besides a few hyaline casts. The tests were invariably applied by the "contact" method—which is in my opinion the most delicate mode of testing.

Experiment has shown me that, in delicate testing, heat cannot be safely resorted to without the greatest possible care in acidifying by acetic acid in proportion to the amount of albumen present, and to the specific gravity of the urine.* In regard to this matter the observer must also keep in mind the possibility of confounding an opacity arising from oxalate of lime with that caused by albumen—a fact well expressed by Méhu: "Les urines chargées d'oxalate de chaux, rendues limpides par filtration, deviennent légèrement louches quand on les fait bouillir, même en l'absence de tout leucocyte. L'addition de quelques gouttes d'acide acétique concentré au liquide louche ne lui restitue pas sa transparence première"—*L'Urine*, p. 326. I am satisfied that no test requires more care and caution than heat with acetic acid in the search for albumen in small quantities; and nitric acid, which has a pronounced mucin reaction—producing a quasi-albuminous zone with a solution of pure mucin—and like citric acid, apt

* This fact has been clearly placed before you by Dr. Kirk, and my own observations confirm it.

to induce a reaction with urates, especially in concentrated urines, is by no means trustworthy in delicate testing. The mode of observation which I prefer to others is to determine, first of all, the presence or absence of mucin by using citric acid *; and then to run upon the acidified urine a weak solution of the reagents—mercuric, tungstate, or ferrocyanide†—selecting the last or the tungstate if mucin has been detected, because these tests give the least reactions with solution of pure mucin; and, finally, to appeal to the miscroscope.

Is the detection of these traces of albumen of any clinical value? The positive answer must still be left to further experience of the chemical tests by the side of the microscope. In the meantime I can corroborate the assertion of those who speak of small quantities of albumen as of frequent occurrence in the urine of persons who present no ordinary signs of disease; but, until it can be shown that leucocytes (possibly indicating, as a rule, some unfelt transient or more continuous catarrh of the urinary mucous membrane) are normal constituents of urine, I cannot see my way to accepting the theory of "physiological albuminuria." Possibly many of these cases of minimal albuminuria bear a similar pathological relationship to confirmed urinary and renal disease, as do passing catarrhs to chronic bronchitis and chronic pneumonia; and it may be that just as these respiratory diseases sometimes develop from repeated catarrhs, so, perhaps, chronic renal disease may now and then become insidiously established out of a symptomless chronic pyelitis, which, in its turn, may have grown out of repeated slight catarrhal inflammations, set up by some irritating quality of the urine, or by some of the constituents (such as oxalate of lime or uric acid) of this chemically complex fluid falling out of solution in the higher parts of the urinary apparatus. I am inclined to think this is a very probable mode of origin of many cases of Bright's disease; it is, perhaps, the rule in the gouty kidney; and why should there not be a "medical" kidney as well as a "surgical" one, from extension upwards, of inflammation of the urinary mucous membrane? I, therefore, look upon it as a matter of some practical importance to examine the urine

* A citric test paper dropped into an inch column of urine, and left to stand for a few minutes, is the most convenient way of testing for mucin; when this body is present in quantity, a slight milkiness gathers in the lower half of the urine.

† A test paper dropped into, and allowed to rest for a minute or so in 20 minims of water, provides the required solution in a ready way.

FAT-EMULSIONS.

(*Cremor Hordeatus Loeflundi*).

The high value of Cod-Liver Oil in the treatment of phthisis is a widely recognised fact. If the wasting consumptive can be enabled to assimilate fat the progress of the disease can usually be stayed.

A certain amount of fat is essential for the building up of healthy tissue. When fat can be taken, and digested, the downward progress of the case of phthisis is reversed; and an upward direction given to it. Wherever there is defective tissue-development, that is in the wide class known medically as the 'Strumous,' the *assimilation of fat is the aim of the physician.* This applies as much to defective bone-development in the strumous child as it does to the development of lowly tissue-growth in the lungs of the adult, otherwise tubercle, or phthisis pulmonalis. The introduction of Cod-Liver Oil has been a boon to this large class of individuals. Why? Because it is so easily assimilated. Not because there is any magic virtue in it. By its means many persons are enabled to take that fat requisite for healthy tissue-formation, who cannot digest other fats. The child which turns away with loathing from a piece of fat upon its plate will eagerly swallow the fishy oil. It is not a question of palate. This consideration will tell us of the importance of fat in the food.

How is fat digested? Fat undergoes no change in the digestive act except that of emulsification, *i.e.*, of division and sub-division, until the particles are minute enough to enter the mouths of the lacteals in the intestinal villi. Cod-Liver Oil undergoes this process of emulsification more readily than any other fat; though some other fats are perhaps more desirable for tissue-building if they could be assimilated. Nature furnishes a fat-emulsion in milk for the nutrition of young animals; and this is much more palatable than the fish-oil. If other fats can be taken, and digested, there is no advantage to be gained by resort to Cod Liver Oil.

Many persons cannot overcome their repugnance to oils, who can take milk, cream, or butter. Then there are cases where the oil is not assimilated, but is voided per anum unchanged and *en masse.* In these last cases the fat must be taken in the form of an emulsion, so that the feeble digestive powers can deal with it. The importance of fat in the form of an emulsion needs no further illustration.

Fat emulsions are made artificially, but they do not keep well: and in time separate. Cream cannot always be had, and preserved cream has not hitherto been made a marketable article. Milk is apt to turn sour; to say nothing of many persons disliking it. It is rather a dilute form of fat; and therefore not suited to some cases. It is, too, a ready vehicle for zymotic disease, and therefore it is not safe to drink it promiscuously. A fat-emulsion which will keep, which is readily digestible, and which can be procured in any quantity, has long been a desideratum with the growing class of persons whose fat-digestion is imperfect.

Mr. Edward Loeflund has met the difficulty in the following manner: The cream is taken from the best milk of the slopes of the Bavarian Alps, renowned for their herds. The cream is concentrated in vacuo, and preserved by the addition of a certain proportion of maltose. The compound is rich in fat; is acceptable to the most fastidious palate, being really, in taste, a confection, while the emulsion form of fat is not destroyed by the evaporation process. For those who cannot take Cod-Liver Oil this preparation is of priceless value. Unfortunately it cannot be produced cheaply. It is put up in small tins, sufficient for three or four days; and will keep for a week without change after the tin has been opened. By laying in a stock of tins the imperfect assimilator of fat can travel anywhere, or go to sea, without being deprived of the necessary fatty food.

There are a large number of persons to whose stomachs fat in any form is repugnant. Their tissues stand in need of fat, but the stomach resents its presence. For them it is essential that fat be taken in a manner which will not offend the stomach. Consequently it must be taken when the contents of the stomach are passing out of it. Fat is not affected by the stomach, and undergoes no change therein; its digestion does not commence until the stomach has been passed. Consequently, the fat must not be taken till the time of its digestion has arrived. This may be taken at from two to three hours after a meal. Introduced into the stomach at this time, it quickly passes out of it. The stomach is thus not offended by fat floating about it while its own proper work is going on; nor is there any splitting up of the fat into offensive fatty acids. Cod-Liver Oil, or cream, or other fat-emulsion taken at this time is not objectionable to the stomach, and so is not repulsive. For those who cannot take Cod-Liver Oil, or where fresh cream is not procurable, this palatable fat emulsion is invaluable. It can be taken alone, or spread on bread or biscuit.

It is valuable during the hectic fever of phthisis; and in summer time, added to *iced* coffee, cocoa, chocolate &c., it makes a most refreshing and *invigorating food.* The maltose is itself a food of high nutritive properties, and the combination is a *perfect food rich in fat.*

The dairies are under regular inspection, and the water supply is both microscopically and chemically examined by a competent medical man and a scientist, so as to guard against any possibility of zymotic germs finding their way into the preparation.

Being free from *cane sugar*, this preparation can be taken by persons who cannot take 'sweets' ordinarily, from the acidity to which they give rise.

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for indications of catarrh, especially of the pelvis of the kidney—mild pyelitis often existing without symptoms; and I am persuaded that the intelligent use of the delicate albumen tests, *along with the microscope*, should aid this inquiry, and, further than this, should prompt us to resort to suitable therapeutic measures at a time when they may avert serious mischief.

3. *Quantitative albumen.*—I have often thought that there are several clinical and pathological problems, in connection with albuminuria, which should derive some light from a more frequent resort to the quantitative determination of albumen; such, for instance, as the effects of diet and climate, of remedies, of Turkish and other baths, and of passing derangements, which are either incidental or are bound up with the pathology of the case, such as the curious bilious attacks to which certain albuminuric patients are liable.* But, hitherto, the difficulties or the inaccuracies of the methods for the frequent quantitative estimations of albumen have doubtless thwarted inquiry. I have, however, lately adopted with much satisfaction a quantitative procedure, which requires but two or three minutes for each observation. I will not occupy your time by describing it, but must refer you to the *Practitioner* of February last.

Prognosis Illustrated—Causes of Intermittent Albuminuria also Operative in Structural Disease of the Kidney.

By Dr. FRANCIS HENDERSON.

On the second evening of the discussion, Prof. Gairdner told us that his experience of albuminuria had led him to form a somewhat more favourable general prognosis than that commonly entertained; and Dr. Mahomed of London, speaking after him, corroborated this view. Working in a comparatively small field, my experience has led me to the same conclusion. I have met with several cases of albuminuria which were both severe and protracted, and yet terminated in complete recovery. These cases belonged to different clinical groups. The following is the summary of

* In reference to these fits of digestive trouble, I have distinctly noted, not only a relative, but a decided absolute increase of the albumen in the 24 hours' urine.

one of these cases, and I have selected it not only because it is a striking example of permanent recovery, but also and chiefly because when studied along with other cases of the same class, it is fitted, I think, to throw some light upon the causation of albuminuria.

In the summer of 1870, a gentleman, aged about 30, consulted me. He complained of weakness. He was much emaciated and very nervous. There was consumption in his family. I examined his chest as carefully as I could, but found no signs of disease. Subsequently I obtained a specimen of the urine and found a large quantity of albumen; but in other respects, as regards acidity, colour, and specific gravity, it was not abnormal. The patient remained for eight or ten days in my neighbourhood, and I saw him and examined his urine on several occasions. I ascertained that the urine passed in the morning was free from albumen, but that passed in the forenoon was highly coagulable. I observed also that his heart was in a constant state of over-action, and although there was no fever present, his pulse was always above 100. He had a very large appetite, and it appeared to me that his blood was loaded with the products of imperfect digestion. After ten days or so, this patient went to reside in Edinburgh, and getting no better, he consulted the late Dr. Warburton Begbie, who gave the case much careful consideration. Dr. Begbie noted the intermittent character of the albuminuria, and clearly made out its association with the periods of digestion. He also observed the cardiac over-action, and he discovered a soft pulsating swelling of the thyroid gland and some prominence of the eye-balls. An account of this case, along with others of a similar character, was published by Dr. Begbie.* For many details I must refer those interested to this article. I shall just state the broad facts.

First, as to the duration of albumen in the urine. Dating from its discovery until its final disappearance, *two years* elapsed. Probably it was present for a considerable time before it was discovered, because at that time the patient was much emaciated; and that the loss of albumen was the chief cause of the loss of flesh is rendered more than probable by the fact, that when the loss of albumen finally ceased, he immediately began to regain flesh.

Second, as to the amount of albumen. The urine passed in the morning and during fasting was free from albumen,

* *Edin. Medical Journal*, April, 1874. "Albuminuria in cases of vascular bronchocele and exophthalmos."

but that passed during and for some time after digestion, particularly after breakfast, was highly albuminous—the quantity varied from $\frac{1}{4}$ to $\frac{1}{3}$ of the column of urine in the test tube; sometimes it was much greater. During the winter of 1871-72 (18 or 20 months after the albuminuria had been discovered), the patient, at Dr. Begbie's request, consulted Dr. George Johnson of London. Regarding the amount of albumen found on this occasion, Dr. Johnson says—"The urine passed in my room an hour and a-half after breakfast became *nearly solid* from heat and nitric acid."*

In view of the issue of this case, Dr. Johnson's prognosis as that of one of the highest authorities on this class of diseases, is particularly worthy of note. In his letter to Dr. Begbie he says nothing about prognosis, but that it was very unfavourable may be fairly gathered from what he said to the patient himself. I quote from a letter of the patient, who is a highly educated man and an accurate man: "Dr. Johnson's reply to a question I put to him as to his view, impressed me as being decidedly unfavourable. I remember he said 'it was a good symptom there were as yet no tube-casts (or some such term), but that in ordinary cases that would be the next stage.'" It will be admitted that Dr. Johnson would not have spoken thus had he entertained much hope of recovery. On the other hand, it must in justice not be overlooked that Dr. Johnson obtained an exceptionally bad sample of the urine.

The treatment of this case consisted in rest from mental exertion and relief from responsible duties, change of climate, and the use of certain drugs. Some of the details can be found in Dr. Begbie's paper. What I desire to draw attention to, is the decided effect of digitalis in arresting the escape of albumen. The patient's statement, which I quote, is in harmony with Dr. Begbie's observations and conclusion: "During my year of rest I had gained strength, but the albumen had not diminished materially, until seeing Dr. Begbie again in May 1872. He then directed me to try, with care, pills composed of 1 grain of powdered digitalis, 1 grain of sulphate of iron, and $\frac{1}{4}$ grain of capsicum—twice daily. From the time I took them the albumen *at once* began to lessen rapidly, and in a month or six weeks it was about quite away, tending to return a little if I were overworked. At the same time my pulse, which had always been a very rapid one, became slower and more natural."

*Dr. Johnson's letter to Dr. Begbie regarding this case is given in Dr. Begbie's article, *loc. cit.*

From this period, this gentleman steadily went up hill, and with the exception of occasional temporary indispositions, has ever since been able for his duties. I have very frequently seen him during these 12 years. He never was a very robust man, but he is in good health. As a proof of the change in his condition I may mention that his *minimum* and *usual* weight during his illness was 8 st. 12 lbs. (this for a man 5 feet 11 inches is very low.) At the present time he weighs 12 st. 3 lbs. After the cessation of the albumen he gained weight very rapidly. It will be admitted that this case is a very encouraging one as bearing upon the prognosis of albuminuria, and it is particularly important because the accuracy of its principal facts cannot be questioned. *

I wish now to make a few remarks regarding the *immediate* cause of albumen appearing in the urine, as suggested by a study of this case and of the records of other cases of *intermittent* albuminuria, of which class of cases this was an aggravated example. It will not be held, I think, that the existence of vascular bronchocele gave an essentially different complexion to this case. It merely showed conclusively the deranged condition of the vasomotor system, and it also served as a visible index of the probable state of the renal blood-vessels. For it may be fairly assumed that the blood-vessels of the kidneys in this case were in the same condition as those of the thyroid gland—viz., relaxed and dilated. And further, I think it will be admitted that this state of the kidney vessels was the *underlying condition* which led to the occurrence of albuminuria. But it was not the *whole* cause of

* Since the above was in type, I heard from this gentleman that he was suffering from an attack of irritability of the bladder. He sent two specimens of urine—No. 1, passed on rising in the morning—No. 2, passed an hour and a-half after breakfast. Both specimens were highly acid and contained large numbers of uric acid crystals. No. 1, sp. gravity 1020. Heat produced a *very slight* cloudiness, which was unaffected by adding a few drops of nitric acid. No. 2, sp. gravity 1022. Heat produced rather more cloudiness, which was also unaffected by nitric acid. After some minutes the cloud separated into flocculent particles, like albumen. This appearance did not occur in No. 1. Nitric acid in the cold urine gave no indication of albumen. As these signs were somewhat doubtful I asked Dr. Robert Kirk to examine the urine. He reports "the morning urine contained a very minute trace of albumen and a good deal more than the average amount of mucin, which made the detection of albumen a matter of some difficulty. The urine passed after breakfast contained less mucin and a little more albumen, but not more than might be described as a trace."

The existence of a trace of albumen in this gentleman's urine, in connection with an attack of irritability of bladder, apparently due to the passing of a large quantity of uric acid, does not impair the conclusion that his recovery from his former illness is practically complete.

the albuminuria—nor was it the *immediate* cause. Had this morbid state of the vessels been the *whole* cause, the albuminuria would have been constant, but it was not. Albumen was only present during and for a time after digestion. What, then, was the *immediate* or proximate cause of the escape of albumen? It was the increased amount of blood in the kidney which normally accompanies the increased activity of an organ. During and after digestion the kidney is in a state of increased activity. More blood in an organ implies enlarged and distended blood-vessels. *Enlarged* blood-vessels imply, *cæteris paribus,* increased blood pressure. *Distended* blood-vessels imply thinner and more pervious walls. There can be little room for doubt that, in the case which I have related, the relaxed and weakened renal vessels yielded too much before the strain of this physiological afflux of blood; that they thus became *over-distended,* and the albumen escaped through their walls. This was the view taken by Dr. Begbie.

Now, if we turn to the records of the "Albuminuria of Adolescence," and other cases of intermittent albuminuria, we find no doubt that there is generally some sign of lowered health or vigour—such as a certain degree of nervous debility, or of vascular strain, or anæmia, or some unhealthy condition which probably forms the basis of the disorder. But putting these aside I ask, What is the *immediate* cause of the albuminuria in nearly all, if not in all of these cases? It is the increased amount of blood in the kidney. This increased amount of blood is the direct result of the following causes, which may act singly or in combination.

1. The physiological afflux of blood in connection with digestion.
2. Causes which diminish the amount of blood in the skin, such as getting out of bed, or cold bathing.
3. The erect posture.
4. Active exercise.

Let me shortly refer to some of the records of these cases in illustration of this. Dr. Clement Dukes of Rugby* stated in 1878 that he found albuminuria very common among the Rugby boys, between the ages of 14 and 16. In some cases "it is most persistent, except the patient be placed on milk diet, in bed and between blankets, when it disappears at once, but may reappear at any moment, on getting him up and letting him have meat." "After a time," continues Dr. Dukes, "when warm in bed he can eat a full meat diet, and

* *Brit. Med. Journal*, 1878. Vol. ii, 794.

yet not produce the faintest trace of albumen, though every drop of urine passed is examined."

Again, the experience of Dr. Rooke* of Cheltenham, among girls between 14 and 16, is very similar, although he gives a different reason. He says—"If such cases be kept in bed or strictly in the recumbent position, the albumen will disappear altogether." "That the albumen can be made to appear and disappear in such a singular manner, simply according to the position, proves almost indubitably," says Dr. Rooke, "the mechanical nature of the affection. . . . When the body is in the upright position, the weight of the column of blood is too much for the weakened vessels."

Dr. George Johnson says†—"I have often met with cases in which the urine has been albuminous *only* after food and exercise;" and Dr. Mahomed told us the other night, speaking of cases of intermittent albuminuria, that albumen will generally be found in the urine passed after breakfast, if it is present at all. I need not multiply quotations, the drift of all experience is the same; and if there are any cases of intermittent albuminuria which seem to be exceptions, it must be remembered that there may be, and probably are, other temporary causes of increase of blood in the kidney. For example, menstruation is believed by some to be accompanied by congestion of the kidney.

The fact which I wish to bring into prominence is this, *that in cases of intermittent albuminuria the times of the appearance of albumen in the urine coincide with the times of increased fulness of blood in the kidney. When the increased flow of blood ceases, the albuminuria ceases.* We are therefore warranted in regarding the increased fulness of the blood-vessels, or the effects of this fulness as the *immediate* cause of albuminuria in these cases. The most natural conclusion is, that the escape of albumen is from leakage through vessels which have become over-distended, from yielding too much before the strain put upon them. This excessive yielding may be due either to the vessels themselves being too weak, or to the blood pressure being too strong, or to both causes combined. If this is the true explanation of what has been called "physiological albuminuria," the albuminuria of adolescence, and of such cases as the one I have related, what ground is there for holding that it is *not* also, at least in part, the explanation of the albuminuria which accompanies kidney disease? If the kidney is structurally diseased, is it

* *Brit. Med. Journal*, 1878. Vol ii, 596.
† *Edin. Med. Journal*, 1874. Vol. i, 883.

on that account, it may be asked, not liable to the influence of these causes and their effects? I am not aware that this subject has been particularly investigated clinically; but I would suggest that in cases of undoubted kidney disease with albuminuria, it would be important to ascertain, by careful observation, what are the effects of those causes which increase the amount of blood in the kidney upon the quantity of albumen in the urine, viz.—the effects of food and of fasting—the effect of the upright posture compared with the horizontal—of the warmth of bed *versus* moderate exposure of the surface—of exercise *versus* rest. In structural disease of the kidneys there are, in addition, local causes, in the shape of exudations, interstitial growth, &c., which, by obstructing the circulation in certain blood-vessels, must give rise to over-distension in others and thus lead to escape of albumen. These *local* causes of over-distended blood-vessels will, of course, continue to a certain degree in operation, although all the *general* causes which increase the fulness of the renal circulation are shut off. So that investigations of the kind I have referred to, might help to determine, in certain cases, what proportion of the albuminuria was due to structural changes in the kidney, and thus some light upon diagnosis, prognosis, and also upon treatment might be obtained. For example, I have notes of a case of undoubted structural disease of the kidney, which finally completely recovered (the patient is now, after the lapse of 14-15 years, in the enjoyment of good health). In this case I find it frequently recorded in my notes that the urine passed in the forenoon contained more albumen than the morning urine. Then, after some months (as the case went on towards recovery), I find it stated that albumen was entirely absent in the morning urine, although still present in that passed in the forenoon. Now, what did this signify? I did not understand it at the time, and probably hardly thought of its meaning; but I would now say that this observation indicated the operation of what may be called the *general* causes of albuminuria, in contradistinction to the *local* or structural causes. And I think it may be added, that the more obvious the operation of these general causes in a given case, the more hopeful is the prognosis.

In many cases of structural disease, as, for example, in acute nephritis, the local conditions, we cannot doubt, must be most important factors in causing distension of blood-vessels and consequent albuminuria. But it is a fact worthy of notice that even in such cases the therapeutic means which lessen the amount of albumen are found by experience to be these which prevent the operation of those very causes which we have just concluded, directly produce albuminuria in the intermittent cases. These means are, complete rest—the horizontal position—warmth of bed, reinforced by hot baths and other agents to increase the amount of blood in the cutaneous surface—limiting the diet to milk, stopping all regular meals, and preventing, as far as possible, all physiological affluxes of blood.

I do not wish to say more about the causation of albuminuria in structural disease of the kidney. It is probable that over-distension of certain of the renal vessels is one of the principal causes; but degeneration of tissue, abnormal states of the blood, and failure of the vital forces must also have a share in its production.

There is another argument, however, suggested by the case I have related, which supports the opinion that the escape of albumen into the urine is due to an over-distended state of the blood-vessels. I refer to the very striking effects of digitalis in arresting the albumen. This patient had been ill, and under careful treatment for nearly two years; he had gained strength, but the albumen had not materially diminished until digitalis (1 grain of the powder twice a day) was given. Then, the albumen at once began to lessen rapidly, and in about a month or six weeks was nearly quite away, and as far as is known it never returned. An observation of this kind might not be of much value if it stood alone: but I need scarcely remind the Society that digitalis has long had a high reputation as a remedy in renal dropsy, and also in chronic albuminuria without dropsy, and that it still retains its reputation. It would be easy to bring forward ample testimony on this point.

How does digitalis act? There is much clinical evidence and also some experimental evidence in favour of the view that digitalis (besides its action upon the heart) contracts the small blood-vessels. As regards experimental proof of this, I would refer specially to Drs. Ringer and Sainsbury's recent experiments.* Now, if digitalis contracts blood-vessels, and if digitalis can arrest the escape of albumen, have we not here good ground for the opinion that albuminuria is due, I mean *proximately* due, to over-distended vessels?

These various considerations, as well as others which might be added did time permit, lead to the conclusion, that the escape of albumen with the urine takes place, when certain

* Royal Medical and Chirurgical Society. See *Lancet*, 1st Dec., 1883.

of the renal vessels are distended beyond a certain point: in whatever way this excessive distension is brought about.

In conclusion, I would add that this explanation of the immediate cause of albuminuria is not essentially affected by the unsettled question whether Bowman's or Ludwig's views are correct. If albumen normally passes through the glomeruli along with the water of the urine (which seems extremely unlikely) then the over-distension of the vessels would act by permitting an excess of the normal quantity of albumen to transude, and consequently a larger quantity than the renal epithelial cells could absorb.

Reply on the Discussion.

By Dr. DAVID NEWMAN.

In my introductory remarks, Mr. President, I limited my attention principally to the consideration of the structural changes in the kidney associated with albuminuria, and to some extent, I may say, I regret this. By so doing I may have limited the discussion somewhat, and laid myself open to the imputation of having considered albuminuria too much from the physical standpoint, and it has, to a certain extent, led some of the gentlemen who have spoken to believe that I did not consider albuminuria in its larger and wider sense. When I opened the discussion it was not my object to give an extensive or general summary of what is known on the subject, but rather to limit myself to a few points round which the discussion might centre. Prof. Greenfield, in his able and interesting speech, says that I base my theory of the reabsorption of water and the non-absorption of albumen upon one case of acute parenchymatous nephritis. Now, this is a misconception. I found it convenient to quote cases rather than to enter into a general description of the diseases, because I considered that the cases were typical ones; or, in other words, that they represented the structural changes I had observed, and illustrated the opinions which I had formed from many other cases. In the case of acute parenchymatous nephritis which I quoted, I had the opportunity of making a very careful quantitative examination of the urine for some time, in fact, during the whole course of the case; and I may say just now that unless a case is taken with its clinical as well as its pathological bearings it is of comparatively little value; but the difficulty in doing this is a misfortune from which the pathologist necessarily suffers in our large hospitals, especially in connection with the observation of chronic diseases. We get, for instance, a patient into the hospital when he is very close, or comparatively close to death, so that he is under observation for only a very short time. Another circumstance which must be considered is, that very often there are certain complications that precede death, and cause the urine to present characters which it would not present if we got the case at an earlier stage. This, gentlemen, was one of the reasons why I did not illustrate what I had to say by a very advanced case of chronic interstitial nephritis, but I preferred to take a case in the middle stages of its course. Prof. Greenfield, in his remarks, asks why I did not carry out my arguments in relation to the case of acute parenchymatous nephritis to the stage when the disease became more advanced, and instead of discussing it at the end of the second week, why I did not discuss it at the end of the eighth week, or at the end of the third month. My reason for not doing so was, that at the end of the third month the case has practically ceased to be one of acute parenchymatous nephritis, not only as regards the anatomical conditions of the kidney, but also in respect to the characters of the urine. As far as I have myself observed in cases of acute parenchymatous nephritis which have had a protracted and interrupted convalescence, the urine is not scanty and highly albuminous, as suggested by Dr. Greenfield, but is usually abundant and of low specific gravity. The albumen has, in the great majority of cases, disappeared to a certain extent. Instead of having a diminished secretion of urine, usually at that stage you have the urine considerably increased from what it was at the time of the acute attack.

I agree very much with what has been said by certain speakers with regard to the importance of making a careful microscopic examination of the kidneys, because I find in the Royal Infirmary that, in making the *post mortem* examinations there, a considerable number of the kidneys that appear to be healthy are really diseased, and also some of the kidneys which appear to the naked eye to be diseased are really healthy. It is, therefore, my almost universal custom not to trust to naked eye appearances in examining the kidneys, but to make a microscopic examination even though there may be no reason—clinical or other—to suspect disease of the urinary organs.

The tendency in this discussion has been to consider Bright's disease in its unity—that is to say, not to distinguish between the different forms of Bright's disease. Now, I would be the very last one to argue that it is a common thing to get a pure case of parenchymatous nephritis, or a pure case of interstitial nephritis. I have no intention to say anything regarding the anatomical types and the question of their unity or variety, whether they correspond to various successive phases of one process, or are distinct anatomical states. The theory of unity has in great part been held by French and German physicians, whereas the doctrine of the duality of Bright's disease has been known as the English theory. You usually find in the kidney, as in the lungs for instance, that parenchymatous are associated with interstitial changes, and *vice versa;* but I think it is convenient to classify cases according to the predominating appearances and anatomical types, and also according to their clinical history. Pathologists are apt to have one classification, and physicians another. The tendency is, I think, for pathologists to neglect clinical observations—to neglect the physical signs and symptoms during life; and a great tendency, especially amongst pathologists who devote themselves to the laboratory and have little hospital work, is, I think, to consider pathology only as it appears after death. I consider it a most important thing that pathologists should be familiar with the appearances and symptoms during life, and probably one of the greatest faults in the arrangements of our large hospitals is, that the pathologist is confined entirely to the *post mortem* room, and has very little to do with what is going on in the wards.

Now, there are some questions which have been raised by different speakers in opposition to the views which I advanced at the beginning of this discussion. It is absolutely impossible for me to enter into all these points in detail, but there are one or two to which I should like to specially refer. My friend, Dr. Middleton, has answered a number of the questions which have been raised by Dr. Coats and by Professor Greenfield; and I do not think, after his able reply, with the remarks in which I entirely agree, it is necessary for me to allude to these; but I should like to ask my friend Dr. Coats why it is that egg-albumen appears in the urine? He admits, I think—he will correct me if I am wrong—that egg-albumen can pass through the glomerulus, even in a state of health; but, at the same time, he argues that the covering of the glomerulus has a selective power in not passing serum-albumen. Now, why, in the case of two substances which are so closely related to one another as serum- and egg-albumen, should the glomerulus allow the egg-albumen to pass and keep back the other? I think, arguing from an anatomical point of view, we have no reason to suppose that the thin layer of epithelium covering the glomeruli has the power of entirely preventing the passage of albumen into the first part of the uriniferous tubules, and I am happy to know that several gentlemen, who have spoken during the discussion, agree with me in this opinion. I am very much disposed to look upon the layer of epithelium covering the tuft of vessels as simply a remnant of the fœtal condition.

Dr. Coats, in his very able speech, I think, practically admits that he has not seen this layer himself. He must have seen it in diseased kidneys; but I find that it is exceedingly difficult to see it in the normal kidney. I have observed it in the kidney of the fœtus, but I have never seen it either in the healthy kidney of a child, or in the kidney of a healthy animal. You may find it in patients who die in hospital, but I think we have reason to suppose that where a patient has reached adult life, and has ultimately died perhaps from a chronic disease, the chances are that some inflammatory changes have taken place, and the epithelium lining the glomeruli has thereby become more apparent.

Dr. Coats mentions an experiment, by Bidder, where the kidney was excised, and when blood was passed through the renal artery of the kidney, serum appeared in the urine; and he argues from it that the glomerular function being interfered with the serum was allowed to transude. That experiment may be explained in two distinct ways. You may either say that the function of glomerular epithelium has been interfered with, or you may argue that the epithelium of the renal tubules has had its function impaired. I don't think that experiment proves anything. And further, as to the experiment performed by Senator and quoted by Dr. Coats. Suppose you produce an artificial albuminuria and immerse the kidney in boiling water, the first part of the kidney that is exposed to heat is the extreme cortex; contraction of the extreme cortex takes place, and the probability is that if there is a small quantity of albuminous fluid in the tubules at that part, it is pushed down from the cortex into the lower parts of the tubules; whereas, if the albumen is abundant it at once coagulates and becomes fixed. That, I think, may be the reason why the albumen was found in the straight tubules, but not in the glomeruli unless the albuminous transudation was concentrated. But I don't think these experiments are of much value; they are exceedingly rough. One cannot judge very much from the appearance of a boiled kidney.

Dr. Perry has called attention this evening to the fact—I think very correctly—that cases of amyloid disease are diminishing in frequency. My experience does not go far enough back to enable me to speak to the time before antiseptics were brought into use; but whether or not this change is due to the use of antiseptics in the treatment of surgical cases, and of inhalations in cases of phthisis, or to a diminution in the amount of syphilis in this city, or to changes in the treatment of that disease, I am not prepared to say. But I know that in the Royal Infirmary amyloid disease is very rare. I meet with very few cases of amyloid disease in the *post mortem* room of that hospital.

Dr. Coats, in his speech, spoke about albuminuria and polyuria in this disease. Well, to answer some of his statements, I have brought a number of specimens, and have placed them on the table to-night. The symptoms and changes in amyloid disease are very similar to those in interstitial nephritis; at least, as far as the application of my theory is concerned. You have polyuria, and you have, as Dr. Perry pointed out, a small quantity of albumen during the early stage. I quite disagree with what Dr. Coats said—viz., that there is a large quantity of albumen almost constantly present in the urine. I have tested the urine in a considerable number of cases of amyloid disease, and estimated the amount of albumen, and unless where there has been either interstitial or parenchymatous nephritis to explain its presence, I have rarely found a large quantity of albumen; but I have certainly found a greater amount than what you usually find in an ordinary case of chronic interstitial nephritis.

Dr. Coats.—I should remark that I was quoting Leube, whose authority on urine testing is undisputed. He states distinctly that the ordinary amount of albumen in the urine in cases of amyloid disease is 2 per cent. I did not state it of my own knowledge.

Dr. Newman.—I am glad Dr. Coats has stated that. Well, I differ from Leube instead of from Dr. Coats.

Now, regarding the albuminuria and polyuria in amyloid disease, I think it may be explained in the following way:—I agree with Dr. Coats that the glomeruli are interfered with in this disease; and I also agree with him that the glomerular tension is increased. In the sections which I have placed on the table, you will see that in the great majority of cases the glomeruli are tolerably well injected, though not always fully injected; but in all the sections the capillaries are very imperfectly filled with the injecting mass. This seems to point—I think Dr. Coats will agree with me—to an obstruction on the distal side of the Malpighian bodies. The afferent arteries are affected, but not greatly so. This would indicate an increased tension in the glomerulus, and if we take into consideration the amyloid changes in the Malpighian tuft, we may admit that a larger quantity of albumen passes through the glomeruli than normally. Now, in amyloid disease, the changes in the epithelium do not take place until the disease is pretty well advanced; and from the fact that the epithelium is not impaired, I argue that it is still performing its function, and is re-absorbing at least a portion of the albumen which escapes from the glomeruli, and so the quantity of albumen found in the urine in the early stage of the disease is not great. Then, how do we explain the polyuria? The increased glomerular tension will explain the escape of water to a certain extent; and, as in cases of interstitial nephritis, so we have also in cases of amyloid disease, a material which obstructs the lymphatics and separates the blood-vessels from the uriniferous tubules. In fact, we have amyloid material occupying the lymphatic spaces. You will find in some of the sections which I have placed on the table rings of epithelium, representing the calibre of the uriniferous tubules, and the whole space between these rings is occupied by amyloid material. This will prevent the transudation of the water from the uriniferous tubules back again into the lymphatics or blood-vessels. In interstitial nephritis the lymphatics are greatly obliterated by inflammatory products, and the tubules are separated from the capillaries by layers of connective tissue; in amyloid disease the hyaline membrane is thickened and the lymphatic spaces are occupied by the new material, particularly around the straight and collecting tubes, and so, as in interstitial nephritis, osmosis from the urinary to the vascular and lymphatic systems is prevented, and polyuria results. I admit that in some instances of amyloid disease the quantity of albumen may be large, but in such cases interstitial nephritis, I suspect, will be found to be present along with the amyloid changes.

We cannot shut our eyes to the broad fact that diseases characterised early in their course by structural changes in the epithelium of the uriniferous tubules are usually accompanied by a large amount of albumen in the urine; whereas, in diseases in which the epithelium is not primarily involved the quantity of albumen is small; and again, it is only in diseases where there is some interference with the outflow of blood from the glomeruli, and where a deposit of some new material, inflammatory or other, has taken place between the tubules and the blood-vessels that albuminuria is associated with polyuria.

I have still something to say regarding the views expressed by Professor Greenfield. He, as it appears to me, makes a most extraordinary statement. He doubts that the afferent vessel is larger in size than the efferent. I will read his words in case it may be thought I have misunderstood him. Speaking of me, he says—"He has especially referred to the relative size of the afferent and efferent vessels of the glomeruli as an important factor in their circulation. But it must be remembered that although there is usually one efferent vessel, the exceptions are not rare in which we find more than one." I have searched through a considerable number of sections since Dr. Greenfield made that statement, and I have found only one glomerulus with two efferent vessels, and I have shown it as a curiosity in the next room. Professor Cleland, in his remarks which I have read, says that he has never seen a glomerulus with two efferent vessels. "Again, there is this difference, that the afferent vessel is highly muscular, the efferent mainly elastic. The afferent is probably normally in a state of tonus, and readily contracts or dilates under the influence of stimuli, so that the relative calibre during life cannot be judged from the results of injections." I would like to know how Professor Greenfield would judge of the size of the vessels in the kidney unless they were injected? You can see the afferent vessels pretty easily without injection, but it is very difficult—almost impossible—to judge of the size of the efferent vessel. Then, again: "In the normal condition of circulation, the volume of blood entering the glomerulus must equal that issuing, and the velocity will be, speaking roughly, inversely to the area of section. So that to speak of the circulation in the glomerulus as normally under increased pressure owing to the relative calibre of the vessels is an assumption not warranted by the facts. In other words, it has not been proved that the transudation of water in the quiescent condition requires any greater pressure than does the normal transudation of lymph from the capillaries." No one, as far as I am aware, has said that it requires a greater pressure to cause the transudation of water than of lymph. I do not think this has been argued by any one in the discussion. As to Dr. Greenfield's first statement in that last sentence, regarding the velocity of the circulation, I wish to inquire how the velocity of the circulation through the efferent vessel can be increased unless there is an increased pressure in the glomerulus? Suppose you have a tube of uniform size, and the water in it subjected to a pressure equal to say 30 feet of water, what will happen? The water will have free exit, and the pressure in the tube will be very slight indeed. But if you constrict the end of the tube, as you do in an ordinary fire-hose, you will produce an increased pressure in the tube, and also cause the water to be ejected with greater velocity. The afferent energy expends itself in two ways—first by causing tension of the glomeruli, and second by forcing the blood along the efferent vessel, and the dilute serum through the glomerular loops into the uriniferous tubules. It will therefore be evident to every one that the freer the exit by the efferent vessels the larger will be the quantity of blood flowing through them, and the smaller the amount of fluid that transudes into the Malpighian capsule. But when a certain restraining force is exerted upon the efferent flow, the whole of it is expended in increasing the quantity of serum that filters through the glomerulus, whereas, when there is an increase in the afferent pressure, it is expended in the two ways above mentioned, unless the efferent resistance is equal to the afferent pressure, in which case, it is evident, the whole force will be exerted upon the serum in the glomerulus to press it through the membrane. The amount of fluid that transudes depends as much upon the efferent resistance as upon the afferent pressure. Dr. Greenfield seems to have confused two ideas here—the ideas of pressure and of velocity. Professor Cleland answers this point very well in his address. But then, again, what Professor Greenfield denies for the normal kidney, he assumes as correct for the diseased kidney, for he says—"If, however, the arteries be relaxed, the velocity of the stream issuing through the efferent vessel will be increased, and at the same time the pressure will be raised in the glomerulus, and if this exceeds that in the tubules increased transudation will occur."

Dr. Middleton, in his remarks, called attention to the fact that both Dr. Coats and Professor Greenfield had mistaken the theory which I advanced at the beginning of this discussion. I consider that the lymphatics perform a very important part in connection not only with the functions of the healthy kidney, but in relation to the diseased kidney.

I think that in health the lymphatics have a great deal to do with the absorption of water, while the epithelium has the vital function to perform; that is to say, it has to select out the albumen, and carry it back into the circulation.

As to albuminuria in heart disease, I agree with a great deal that Dr. Coats and Dr. Steven have said. That the afferent pressure *per se* is not sufficient to cause albuminuria I admit, and I consider that the efferent resistance is of far greater importance. Let us suppose that there is some obstruction to the onward flow of blood through the veins. What will be the result? The organ will swell up enormously, the veins will become gorged with blood, and the tension of blood in the glomeruli greatly increased; at first there will be a rapid filtration of the fluid constituents of the blood into the capsule of the Malpighian body; but at the same time as this is going on, the network of veins surrounding the uriniferous tubules will become dilated to an enormous extent, press unduly upon the tubules, and in this way cause obstruction to the onward flow of the urine. When the tension of the fluid in the tubuli uriniferi increases, the amount of urine secreted diminishes, and when it becomes equal to the arterial tension the secretion ceases, and the circulation through the Malpighian body may be stopped altogether by collapse of the arterial coil when the pressure outside is greater than that inside the vessel. If this happens it can be easily seen how, by damming up of the urine, atrophy of the substance of the kidney may be produced as a result of insufficient blood supply, seeing that most of the cortex is nourished by the efferent vessels of the glomeruli. It is found as a clinical fact that diminution or suppression of urine may be caused by changes in the structure of the glandular substance (tubular nephritis), where the onward flow of the urine is prevented by blocking of the tubuli uriniferi with fibrinous exudation, blood corpuscles, and epithelial cells. As a result of this, the tension of the urine in the capsule of the Malpighian body will be gradually increased until it becomes equal to the tension in the arteries, when the secretion will cease. But if from any cause the tension outside becomes greater than that inside the vessel, it is evident that the glomerulus will collapse, and the onward flow of blood be prevented, unless the urine be reabsorbed. Congestion not only produces an increased pressure in the glomerulus, but it also causes an impairment of the nutrition of the whole kidney, particularly of the epithelium, and it is by this interference with its nutrition that the epithelium is no longer able to perform its function, but allows the albumen to escape.

I am sorry that Dr. Coats occupied so much of his speech by the quotation of experiments. I would have greatly preferred to have heard his own views based on his own experience, because there are few pathologists in this country able to call up so extensive and various experiences as Dr. Coats. The experiments quoted by him are extremely interesting, but they seem to me to prove very little, because they are all capable of a double interpretation. All these experiments in connection with blood pressure—experiments based upon ligature of the renal vein and of the renal artery, or of the ureters—may be interpreted in two ways, and unless we are able to perform some experiment, and by eliminating all other possibilities prove one theory to be right and the other wrong, I am afraid the statement of our views regarding the functions of the glomerulus must remain purely theoretical. What we have to do at present is to select the theory which will explain the circumstances, not only of health, but of disease most fully, and I hold that the theory which I have adopted does this—a theory which, by the way, is not Ludwig's theory; it differs from Ludwig's theory in a number of respects. It differs from Ludwig's theory in the fact that I hold that the albumen filters through the glomerulus. Now, the theory which is generally described as Ludwig's is, that the serum of the blood, less the fats and the albuminates, filters through the walls of the glomerulus into the capsule of the Malpighian body, the filtrate then passes along the convoluted tubules, and comes in contact with the epithelium and the lymph contained in the lymphatic spaces surrounding the tubules; part of the water of this dilute filtrate is then reabsorbed. This is believed to take place by a process of diffusion between the fluid in the tubuli uriniferi and that contained in the lymphatic spaces and veins surrounding the tubes on all sides. I believe that Ludwig at one time held that albumen passed, but he has given up that portion of his theory. Now, I think it is a pity Ludwig gave up that portion of it.

Dr. Coats—That is rather against you though.

Dr. Newman—That is nothing.

Dr. Roberts, in his very interesting remarks, introduced the term "physiological albuminuria." Most of the members and visitors who have spoken since have found fault with the name; and I am certain every one will admit that it is a bad one; but probably by giving it that designation he has caused more discussion than he would if he had given it the more correct name. I consider in that respect it is rather fortunate he did call it physiological albuminuria, because there is a tendency, I think, among medical men to look upon albuminuria as a symptom too seriously; and his bringing it under our notice under the name of physiological albuminuria has led us to discuss it very fully indeed, and has to a certain extent influenced our minds in favour of a better prognosis in certain cases.

Now, gentlemen, I have many other points which I would like to refer to, but I fear I have taken up too much of your time already, so that I will not say anything further than that I think we may congratulate ourselves that the discussion, so far as it has gone, has been successful in bringing forward a great number of very interesting points, and certainly it will act as a great stimulus to future thought and work.

Concluding Remarks by the President.

Professor M'Call Anderson said—Gentlemen, In bringing this discussion to a close, I feel that I am in the happy position of the clergyman in the pulpit, for no matter what I may say, no one will this evening have the opportunity of contradicting me. All, I suppose, will agree that it has been of a most interesting and instructive character; and although no new incontrovertible fact has been brought forward, I am sure we have all got much food for future reflection and investigation.

I must confess that when I received the billet of our energetic Secretary, stating that I was to sum up the discussion, I felt rather staggered. This would be a Herculean task which, you will be glad to hear, I have no intention to undertake; but there are a few points which occurred to me as it went on to which I may shortly refer.

There was one point specially which struck me throughout, and that was the extreme diversity in the opinions which were expressed. Indeed, I think I am correct in saying that there is hardly a single statement which has been made by any speaker which has not been politely but firmly contradicted by those who followed him; and I shall illustrate that by a few references to the pathological side of the question.

The discussion was opened by Dr. Newman, who stated the view that in health not only water, but also albumen passes through the glomerulus,—that it is taken up into the circulation again through the medium of the epithelial cells which line the uriniferous tubes; that in tubular nephritis a number of the epithelial cells are destroyed or their functional activity so impaired that they are unable to take up this albumen, which accordingly appears in large quantity in the urine; but that in granular degeneration of the kidneys, the renal epithelium is not so seriously interfered with, and therefore albumen, if present at all, is only found in small amount. This view of Dr. Newman's has not been accepted by my friend, Dr. Steven; but I shall not refer to his views, because they are so fresh in your minds that it is not necessary for me to recapitulate them. Dr. Mahomed also disagreed with Dr. Newman, and held that the chief factors in the production of albuminuria are retarded circulation, combined with increased pressure in the glomerulus. Dr. Coats, again, admitted that egg-albumen was separated by the glomerulus, and that in amyloid disease, the diseased vessels of the glomerulus—as well as the arteriolæ rectæ—allow the albumen to escape; but he holds that serum albumen never passes through the glomerulus in a state of health, but only water and salts. On the other hand, Dr. M'Gregor-Robertson, in his interesting and incisive address, gave it as his opinion that Dr. Mahomed had altogether confounded two distinct processes—those of filtration and of osmosis—that while albumen will not pass through a membrane by osmosis, it passes through readily and of necessity by filtration; and that as the process which takes place through the glomerulus is one of filtration, therefore, as a matter of course, the albumen must filter through; and not only albumen, but also all the constituents of the blood plasma, although in very different proportions from those in which they exist in the blood. He agrees with Dr. Newman also that this albumen which escapes through the glomerulus in health is absorbed partly by the epithelium of the convoluted tubes, and partly by that which lines the looped tubules of Henle, which are contracted, and where therefore the flow of urine is considerably delayed.

Dr. Middleton also adopts the view which has been stated by Dr. Newman; but from the remarks which you have heard from the latter gentleman this evening, you will readily understand that he is quite able to support his own position.

Dr. Finlayson limited his observations to a consideration of the production of albuminuria owing to the presence of poisons in the blood, of which we have the most familiar illustration perhaps in the case of diphtheria. Quite recently some interesting experiments have been made by M. Bouchard on the introduction of chloroform into the sub-cutaneous cellular tissue of the lower animals. He found that if he inserted a cubic centimetre of chloroform into the cellular tissue of dogs or rabbits, they did not appear to suffer at all for 24 to 36 hours, but at the end of that time a large amount of albumen was found in the urine, and the animals rapidly succumbed. In one class of those cases referred to by Dr. Finlayson—namely, the acute infective processes, where the poison induces fever—I am of opinion that the albuminuria is often not dependent directly upon the poison in the blood, but upon the fever which has been induced by the poison. There is no doubt whatever that high fever is of itself sufficient to produce albuminuria—the kidneys partaking in the general congestion of the system; and you will find in not a few of those cases that if, by means of antipyretics, you bring down the fever, the albumen not infrequently disappears, although the poison in the system remains.

Dr. Roberts has very strongly insisted upon the existence of normal, or, as he terms it, physiological albuminuria, a view which seems also to be taken by Dr. Mahomed, for, amongst other things in his paper, I find a statement to the effect that in 58 cases in which he examined applicants for life insurance, he found albumen present in the urine in 9. But I am not very clear that I understand exactly what he means, because he states that these 58 cases were "thought to be healthy;" but in 7 of them they were "clearly not in perfect health;" and also, I am not quite sure whether the 58 were consecutive, or selected cases. If they were consecutive cases all I can say is, that his experience is entirely opposed to mine. For the last few years, in connection with some of the offices for which I act, it has been made imperative that an examination of the urine shall be made in every case as a matter of routine; and since I began to do so I have never once found albumen in the urine, unless in cases in which, from their other surroundings, I should, as a matter of course, have examined the urine. This is also the experience of Professor Greenfield, who acts for one of the largest insurance companies in Scotland; and of Dr. Scott Orr, who acts along with me in one of the offices referred to. I am therefore inclined to agree with my colleague, Dr. Gairdner, in having very grave doubts of the existence of the so-called physiological albuminuria.

There is one point which has hardly been alluded to at all in connection with this discussion, or only indirectly, and that is, albuminuria occurring as a neurotic affection. It is very probable that those cases of albuminuria occurring in young adults—male and female—at the period of puberty, are really instances of neurotic albuminuria. But the best illustration we have of it is to be found in cases of exophthalmic goître. It is now well known that in that disease albumen in the urine is, though not a constant, yet a very common occurrence; and when it coincides with well marked dropsy, one would be very apt to suppose that such a case of exophthalmic goître was complicated with tubular nephritis. But it is not so. The dropsy is produced by the same vasomotor nerve disturbance which produces the albuminuria. Sometimes we find both albuminuria and dropsy; sometimes the albuminuria without the dropsy; and sometimes the dropsy without the albuminuria. In the last two cases of exophthalmic goître which have come under my notice, in neither was there albumen in the urine; but in both there was very well marked dropsy of the lower extremities. Several years ago I attended a young lady, who had well marked exophthalmic goître; she ultimately died, owing to the complication of phthisis. Two years ago I was consulted by her sister, who likewise had well marked exophthalmic goître. She had no albumen in the urine, but she had well marked dropsy of the lower extremities. This case fortunately terminated in a different way; it resulted in perfect recovery, and as the other symptoms of the exophthalmic goître disappeared the dropsy of the lower extremities likewise vanished.

There is another point which I think has not been sufficiently attended to; and perhaps it is just as well that I should state it, seeing that in this discussion our attention has been so much directed to the presence of albumen in the urine, and that is the danger, in the diagnosis of the presence or absence of Bright's disease, of concentrating our attention too much upon the presence or absence of albuminuria; because some of the worst cases of chronic Bright's disease are those in which no albumen is to be found in the urine. I quite agree with what has been stated by Dr. Newman, and also by Dr. Perry, that in cases of amyloid disease of the kidneys, Leube is incorrect in saying that albumen is usually present in large quantity. I quite admit that in the later stages of amyloid disease of the kidneys albumen is often present in large amount; but if you see the case in the early stage, when the patient is in failing health, and is passing a large quantity of pale urine of low specific gravity, very often you cannot find a trace of albumen in it.

The classical symptoms of Bright's disease are albumen in the urine and dropsy. But in granular degeneration of the kidneys it happens that very often we have no dropsy whatever from first to last, and we cannot find any albumen in the urine, and yet these are the most hopeless cases in the long run; and I am afraid that pathologists can tell us that, in making *post mortem* examinations, it is the most every day occurrence to find more or less evidence of granular degeneration of the kidneys, when there was not the least suspicion of such a disease during life.

The most important point in connection with Dr. Mahomed's paper was that in which he drew attention to the necessity of attending to the blood pressure, for I conceive that to be one of the most characteristic features in the symptomatology of Bright's disease. I quite agree with him that albuminuria, without high tension, is a comparatively favourable occurrence—much more so than when the two conditions are combined. I also agree with him in thinking that high tension often precedes Bright's disease. I will give you an illustration. A patient is sedentary in his habits, and fares sumptuously every day; excrementitious matters accumulate in the blood, and high tension is the result. Put that patient under treatment—regulate his diet and regimen, and stimulate the organs of excretion, and the high tension will disappear. But if, instead of that, he goes out during an east wind, such as is now prevalent, and catches cold, it is not unlikely that an attack of tubular nephritis will result. So that it is of the greatest importance that we should not only attend to the symptom of albuminuria, but also to the indications afforded by the pulse.

The usually received opinion with regard to the cause of high tension is, that the blood is in a more or less poisoned condition, that it irritates the small arteries and capillaries throughout the system, and causes them to contract, that the left ventricle acts more energetically in consequence, and a condition of high tension is established. But the other night Professor Hamilton astonished us with a novel and ingenious theory as to its production. He told us that, in a state of health, the specific gravity of the red corpuscles is practically identical with that of the serum of the blood, and that therefore they glide along in the axial stream, and there is no friction; but let the specific gravity of the serum of the blood be lowered or raised, then the red blood corpuscles will run along the lower or upper surface of the vessels, particularly at their bends, and high tension is the consequence. This view was not exactly accepted by Dr. Leishman; but he seemed to think it a probable one, and stated that it was supported by the fact that in pregnancy the specific gravity of the serum of the blood was lowered, and that in that condition albumen was very frequently present. Now, there are certain arguments against Professor Hamilton's theory. One of these has been referred to by Dr. Mahomed. He pointed out that the lowering of the specific gravity of the serum of the blood was a slow process, whereas, the development of high tension, as we know, is often very rapid. It may take place within a few hours; in fact, it may take place when there cannot be any alteration of the specific gravity of the serum of the blood at all. But there are other arguments against it. Supposing a patient has lost a quantity of blood, what is the consequence? The almost immediate consequence is, that the blood absorbs water from the system and becomes exceedingly watery. The specific gravity of the serum of blood is proportionately lowered; and, according to the view of Dr. Hamilton, the red corpuscles ought to rise to the surface, and we ought to find a pulse of high tension, or, at all events, not the very opposite condition. Finally, it appears to me that the specific gravity of the red corpuscles must vary with that of the fluid in which they are floating; so that, altogether, I, for my part, cannot accept this theory of Dr. Hamilton's, although no one can withhold from him their admiration for his great ingenuity, and for the attractiveness of the theory which he has enunciated.

The only other point with which I shall trouble you is that having reference to the tests for albumen; and here we have had nearly as much diversity of opinion as in connection with the other points. Dr. Roberts, who is a very high authority on this subject, and who has himself devised a most admirable test for albumen—the brine test—has given it as his opinion that the more recent tests for albumen, including his own brine test (so that he is perfectly unbiassed), are neither so delicate nor so reliable as our old friends, heat and nitric acid. Dr. Mahomed, on the other hand, does not agree with him, and he tells us that with the newer reagents he has often found albumen in the urine when he could not detect it with nitric acid. Then Dr. Kirk informs us that nitric acid is the most hopelessly inferior of all tests for albumen. He holds that picric acid is very much more delicate, and he has pointed out a fallacy in connection with Dr. Roberts' mode of investigation. Dr. Roberts, you will remember, said that if you go on diluting albuminous urine with water, a point is at last reached when nitric acid still yields a precipitate, while picric acid fails. But Dr. Kirk has very properly pointed out that if you want to get the full delicacy of the picric acid test, you must run the solution along the side of the vessel, so that it will lie upon the surface of the urine. Now, if you dilute the urine with water, as Dr. Roberts did, a point is at last reached when its specific gravity is lower than that of the picric acid solution, so that you cannot carry out the picric acid test on the contact principle; and he has found that if in such a case you add some other substance to the urine, such as salt, so as to raise the specific gravity of the diluted urine above that of the picric solution, you succeed in obtaining a precipitate with picric when you altogether fail to find it with nitric acid.

There is one fallacy in connection with the picric acid test which Dr. Kirk referred to, namely, that mucin may be thrown down by it, although the same remark applies to the nitric acid and other tests; but Dr. Kirk has pointed out a very easy way by which we can correct that fallacy. If we take a solution of citric acid of the specific gravity of 1005 and test the urine with it, and if we get no precipitate in a couple of minutes, whatever else the precipitate with picric acid may be, it is not mucin. There are other two sources of fallacy in connection with this test which may be mentioned. One is, that peptones or vegetable alkaloids yield a precipitate with picric acid. Peptones, we know, are very rarely found in the urine, and the only vegetable alkaloid likely to be administered in sufficient quantity to yield a precipitate is quinine. Now, there is a very easy way of distinguishing such precipitates from albumen, because albumen precipitated with picric acid is not soluble by heat, whereas peptones and the vegetable alkaloids are. There is just one other source of fallacy as far as I know, and it is this, that if you have a great deal of albumen in the urine, and you use very little of the picric acid solution, the precipitate is re-dissolved on agitation, but this is overcome by adding more picric acid, and then the precipitate will no longer disappear when the mixture is shaken. I have used all the more recent tests very frequently in the Western Infirmary and elsewhere. I have tried Dr. Roberts' brine test; I have tried the test with equal volumes of carbolic and acetic acids, the picric acid, the tungstate of soda, and the potassio-mercuric iodide tests, and compared them with heat and nitric acid, and I have come to the conclusion that there is no more beautiful or more delicate test for albumen than the picric acid solution; so that I thoroughly agree with what Dr. Kirk has said, and with the opinion which has been expressed with regard to it by Dr. George Johnson. Gentlemen, I shall not detain you longer; but, in conclusion, I venture to throw out the suggestion that perhaps it might be a good thing, at some future meeting of this Society, to have a discussion upon a point which is of the greatest interest to every medical man—namely, the treatment of albuminuria.

The following specimens were shown by Mr. H. Lyon Smith (Dr. Newman's assistant), illustrative of the lesions found in amyloid disease of the kidney:—

(*a*) Section of normal kidney, showing the Malpighian bodies and capillary circulation fully injected.

(*b*) Amyloid kidney; section showing Malpighian body with one (large) afferent and two (small) efferent vessels. In this section the capillary system on the distal side of the Malpighian bodies is very imperfectly injected, while almost every Malpighian tuft is either fully or partly distended by the injecting material. Stained with logwood and mounted in Canada balsam; × 80 diam.

(*c*) Partly injected Malpighian body with afferent arteriole. Stained with methyl aniline and mounted in acetate of potash. The amyloid material has assumed a pink colour, the injected loops contain carmine mass, while the unaffected structures are of a blue colour. The interlobular artery, and the vessel leading to the glomerulus, do not contain much amyloid material, and the glomerulus is most involved where the loops approach the orifice of exit. The capillaries of the cortex are very irregularly affected, and contain almost no injecting mass.

(*d*) Longitudinal section through the medulla prepared in the same way as section *c*. In this portion of the kidney all the vessels are more or less infiltrated with amyloid material, and very few of them contain any of the injecting mass. The basement membrane of the straight tubules, loops of Henle, and collecting tubes have also become more or less involved, and in some of them even the epithelium has assumed the pink colour indicative of amyloid infiltration.

(*e*) Transverse section through the zone of limitation prepared as the two last sections. (1) The Malpighian bodies are all injected, but not fully; (2) the capillary circulation uninjected, or very incompletely injected; (3) the injection is most marked around the convoluted tubules, but even there is exceedingly scanty. In the region of the straight tubules the vessels are almost entirely occupied by amyloid material, and in only a few instances have they allowed the injecting mass to pass. At many points in the section the lymphatic spaces between the tubules and the blood-vessels have become occupied by amyloid material, while the lumen of the straight tubules is occupied by unaffected epithelium. At some parts of this section there are evidences of sub-acute interstitial nephritis.

(*f*) Transverse section through the labyrinth from same case as section *e*, stained with logwood and mounted in Canada balsam. The section shows the injection of the capillaries around the convoluted tubules as contrasted with those around the straight tubules.

(*g*) Amyloid disease with interstitial nephritis. Stained with logwood and mounted in Canada balsam.

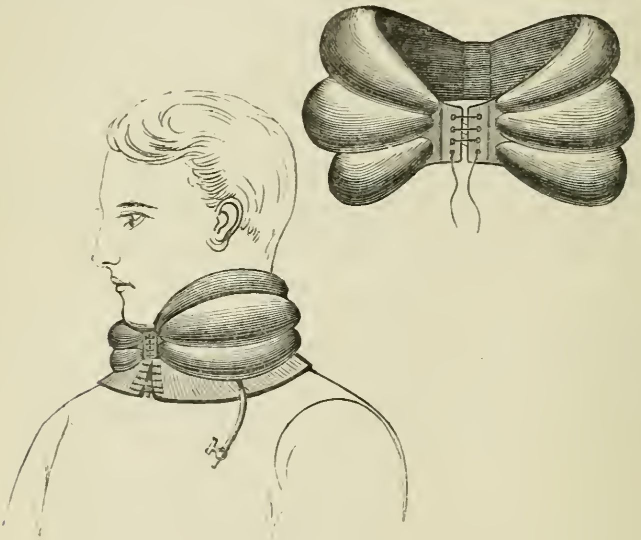
(*h*) Same as the above, but unstained.

ORIGINAL ARTICLES.

ON SUPPORT AND FIXATION OF THE HEAD BY EXPANDING INDIA-RUBBER BAGS IN DISEASE OF THE UPPER PART OF THE SPINE.

By WILLIAM J. FLEMING, M.D.

Hitherto the jurymast attached to a jacket, or the recumbent position, perhaps aided by weight extension, have been the only means at our disposal for the mechanical treatment of disease of the upper region of the vertebral column. I need not dwell on the objections to these methods; all surgeons are only too familiar with them. One day, in Mr. Hilliard's shop, there was shown to me a most ingenious apparatus for the treatment of wry-neck, which at once suggested to me that one on each side would lift and fix the head more effectually and conveniently than the jurymast. Such an instrument I had made and showed at the meeting of the Medico-Chirurgical Society on February 2, 1883. I have delayed further publication of the subject until able to give some cases in which it had been used. The drawing which has been kindly made for me by Mr. Hilliard explains the arrangement almost without verbal description. It consists of two sets of three fusiform india-rubber bags, connected by a narrow flexible but non-elastic material, and having at the free ends flaps, by which they can be laced together. In each set the bags are internally in communication with each other, and from the lowest depends an india-rubber tube with a stop-cock, to which the nozzle of an ordinary small blower fits. A size suitable to the particular case having been selected, it is, while collapsed, laced rather loosely round the neck. On inflation the lower bag rests upon the root of the neck, the clavicle, and the muscles of the shoulders, while the upper bag moulds itself along the posterior portion of the jaw, the mastoid process, and the skull back to the occipital region. The front of the apparatus, where it is laced, is on inflation rather withdrawn from the neck, so that no pressure on the trachea is produced.



In practice, I find it best in the majority of cases to adjust a piece of poroplastic round the neck, like a wide turn-down collar, as shown in the diagram, and allow the lowest bag to bear upon this rather than the skin. If great lifting power is desired, the lateral expansion of the bags may be checked by a net or light bandage, but from their construction the chief expansion is in a vertical direction, so that the net is rarely necessary. Indeed, it is generally easy to lift more forcibly than the patient can tolerate; but the wearers very soon come to manage the distention for themselves, carrying it just to the point from which they derive the greatest advantage. In some experiments which I made by placing the bags round a glass cylinder, over which a broad ring just slipped, I was able to raise over 8 lbs. through a distance of more than an inch. No doubt greater pressure could have been obtained, but the fear of bursting the bags prevented me from carrying the inflation further. By this means, then, I claim that we have the power of taking a large part of the weight of the head off the portion of the vertebral column above its connection with the shoulder girdle, and to a great extent of fixing the head. The apparatus is light, can be worn either in the upright or recumbent position, and when covered with a scarf is scarcely perceptible. No inconvenience has been complained of by the wearers, except that in one case the heat was objected to, but this I hope at least to mitigate by fluting the inner surface. That the whole weight of the head can be borne by the apparatus I do not consider probable, but neither can this be attained by the jurymast, and we know how even the support of the hand in those cases, slight as this must be, gives relief.

For the following hospital cases I am indebted to Mr. H. E. Clark, who kindly allowed me to apply the apparatus to patients in his wards; and to Mr. W. Macknight Wilson, M.B., who has furnished me with the reports, and who has taken great trouble to trace the cases, but, I am sorry to say, only in one successfully.

Case I.—M. M'G., 30, had been under my observation for some years, suffering from various slight strumous affections, and for about two years from symptoms of disease of the cervical vertebræ. Rest, counter-irritation, &c., always relieved her for a time, but the symptoms recurred as soon as she began to go about again, and the confinement was injuriously affecting her general health. As soon as the collar was applied she was able to go about without pain, and while wearing it was much benefited by a visit to the seaside. After a few months she was able to dispense with it altogether, and has since remained well; but on visiting her the other day I found that she was anxious to resume wearing the apparatus, as she again felt slight symptoms of her old complaint.

Case II.—R. P., aged 40 years, a calico printer, was admitted into Ward XX of the Glasgow Royal Infirmary on 11th April, 1883. He is a man of medium height, fairly stout built, and has always enjoyed good general health till lately.

Four or five months ago he began to feel pain in the back of his neck over the upper cervical vertebræ; this feeling, which at first consisted of stiffness, has gradually got worse, and now there is great pain over the region of the atlas and axis. Pain is increased on pressure, especially over the right side of these vertebræ, also when the head is pressed downwards. Any attempt at movement causes pain. The head is bent very much forwards, downwards, and to the left, so that the chin *almost touches* the left side of the chest wall.

From the degree to which the head is fixed in the downward direction, the shoulders appear very much raised, so that he has a decided "round shoulder" appearance.

It is almost impossible for patient to move his head, and in attempting to look to one side or other he has to turn his body round; also, if he attempts to look upwards the shoulders are thrown backwards. The sterno-mastoid and other muscles are not unduly contracted.

12th April.—Fly blister was applied over the right mastoid process.

20th April.—Blister repeated over the same region.

26th April.—Dr. Fleming applied the inflating bag around the neck to raise the head.

27th April.—Pain a little easier.

2nd May.—A poroplastic collar-piece was put on to-day around the neck and partly over the shoulders, and then the air bag applied as well. Patient taught to inflate the bag himself.

23rd June.—Patient has got great relief from the use of the air bag, which he can now regulate quite well for himself. There is still a good deal of bending forward of the head; but the pain which was such a prominent feature is now very much less. So much improvement has been made that to-day he goes to Lenzie Convalescent Home. He takes the apparatus with him.

Case III.—Mrs. M'G., æt. 35 years, housewife, was admitted into Ward XVIII of the Glasgow Royal Infirmary, on 27th June, 1883. About two years ago this woman felt as if she had got a cold in the back of her neck—the neck was somewhat stiff.

This feeling passed off soon to be followed at uncertain intervals by similar attacks. These attacks were at first so simple that she paid very little attention to them, but as one succeeded another they gradually became more marked, and lasted for a longer time. About Christmas time of last year her head became fixed to a certain extent, and since then she has not been able to move it freely: pain has been a marked feature since then, especially on movement. The glands along the edge of the sterno-mastoid then became, and still are, enlarged, and there is a swollen gland in the right axilla.

There is at times pain down both arms, but none in the back or legs. The head is fixed with the chin downwards and towards the right side; and she has pain on pressure over the occipito-atlantoid joints, most marked on the right side.

30th June.—Thermo-cautery was applied to right side of spine, at its upper part.

8th July.—Wound quite healthy: granulating quickly.

12th July.—Dr. Fleming applied the air bag around the neck to-day.

13th July.—In addition to air bag a poroplastic collar was put on to-day.

21st July.—Patient says pain is much less than it was: movement is better. She can adjust the apparatus for herself now, and walk about for a few hours during the day.

30th July.—Improvement continuing.

31st August.—For the past three or four weeks the patient has gone about the ward without almost any inconvenience from the apparatus, which she can now regulate quite easily for herself. To-day she leaves the hospital: there is no pain on pressure: movement is good—her general health is very good.

Case IV.—A. D., æt. 10 years, was admitted into Ward XVIII of the Glasgow Royal Infirmary on 1st August, 1883. Two months ago this girl complained of some slight stiffness in the back of her neck; pain soon followed; this was referred to the upper part of the spine (occipito-atlantoid joint); her head became fixed with the face directed downwards, and to the right side. Any attempted movement of the head from this abnormally fixed position gave rise to pain, as did also pressure of the head downwards. Symptoms of occipito-atlantoid disease were well marked. No undue contraction of the muscles of the neck existed, nor were any of the glands enlarged.

8th August.—Fly blister over right mastoid process.

28th August.—Dr. Fleming applied the air bag and poroplastic collar to-day.

5th September.—Pain is lessened: patient can move about more easily.

10th September.—There is greater freedom of movement; pain is diminishing.

15th September.—Dismissed greatly improved: movement good: pain has now quite disappeared.

25th March, 1884.—The patient was visited to-day: she can move her head in any direction without causing pain: pressure and rotation cause no pain. So much faith have the parents in the apparatus that it is applied now and again.

CASE OF TETANUS.

By J. WALLS WHITE, M.D.

(Read before Glasgow Southern Medical Society, 21st February, 1884).

Tetanus, with its bad fame and its noted fatality, is always a subject of interest and anxiety to the physician called upon to deal with it. It is a disease that oftener comes under the notice of the hospital physician than the general practitioner. Anything that can throw light on its pathology or treatment is always of value. The case I have the pleasure of bringing before you to-night is that of a little boy 5 years of age, a strong wiry little fellow—of healthy parents, with no history of any previous severe illness. For some eight or ten days before I was called to see him, and that was on the 23rd June, 1883, he had been complaining, was peevish, irritable, and out of sorts; had toothache occasionally, and was disinclined for, or unable to eat solid food. In spite of the usual domestic remedies, senna and castor oil, he had gradually become worse; his mother remarked that his countenance had acquired a very peculiar look, and latterly he began to walk with difficulty, and that in a crouching fashion: as he himself put it, he was getting like his Granny. He got always worse at nights. I found him in a state of rigid opisthotonos, the body arched, and resting on the head and heels; his only complaint was of pains over the whole body, especially in the arms and legs; the mouth was open about an inch, and the jaws fixed; all the muscles of the face were in a state of rigid tension, giving to the countenance that peculiar expression so characteristic of the disease. The tongue was small and immobile, very dirty, and the edges ragged and ulcerated, giving evidence of how severely it had been bitten. The pulse was good, and the temperature normal. He was tormented with a short irritating cough, which prevented him from sleeping, and brought on sharp convulsive spasms over the whole body. A careful examination of the body, and especially along the vertebræ, revealed nothing very definite as to the cause of the mischief; but in the region of the upper dorsal vertebræ I thought I could detect a tender spot, but all the parts were so rigid, and the moving and handling of the patient set up such severe spasms, that I could not place much value on this. I at once put him on ten grain doses of bromide of potassium, every two hours, applied a poultice to the throat and chest, and had some warm gruel given him to drink, to allay, if possible, the throat irritation that was so troublesome. But this did no good, as that night proved to be the worst he had ever passed. The spasms were more frequent and severe, his legs and arms were described by the attendants as having become quite knotty, so great had been the muscular contraction; and his mother had no expectations that he could have survived till the morning. When I saw him on the 24th he was in a state of complete exhaustion. I ordered him to be fed on milk and brandy, and put him on a mixture containing 5 grains chloral and 10 grains bromide of potassium to the dose, which was to be given every hour till he slept, and thereafter whenever he awoke. In about twelve hours he had got a drachm of chloral and ʒij of bromide, and under its influence he passed a very quiet day. 5 grains of calomel were given in the morning, which moved the bowels freely; and in the evening a warm water enema was administered, by this means hoping to eliminate one cause of the origin of the evil, if it should be that some gastric disturbance had to do with it. The night following was passed very quietly and without any disturbance; and on the morning of the 25th I found him sleeping calmly, breathing through his nostrils, his mouth closed, and the legs flexed; but the back was still arched and rigid. The medicine was continued, but in smaller doses, and less frequently. During the night, however, he had several severe spasms, biting his tongue till it bled.

On the morning of the 26th, however, he was much improved, could raise himself in bed a little, by first rolling over on his face, and then getting up on his hands and knees: the muscles of the back were still rigid, but he could open his mouth and chew a piece of bread. The tongue he could protrude a little, it was still very hard and dirty; a little solution of potass. chlor. and glycerine was given to relieve this.

Finding that fomenting the throat and clearing out the bowels had not removed the cause, and having strong suspicions that the spine was the seat of the mischief, I now applied some chloroform and liniment of belladonna on lint along the back, but the superficial pain it caused was so great that we had to be content with merely rubbing it on the parts.

On the 27th (the doses of medicine having been reduced) I found that he had passed a very restless night, the cough continuing to annoy him very much, and being accompanied by spasms of the cervical muscles, by which the head was dragged backwards to an alarming degree. To the chloral mixture I now added 5 drops of tincture of belladonna, giving full doses again and giving freely, and by its aid a very quiet day was passed.

On the 28th he was able to eat an egg and drink some tea, but the rigidity of the cervical muscles was unabated. A careful examination of the spine now revealed a very sensitive spot, which felt like a distinct little swelling just below the 7th cervical vertebræ; over this a blister was painted; the chest was firmly bound in a pair of child's stays. Next morning the blister had risen well, the mouth and tongue were much improved, the cough was nearly gone, and the head had come forward to a more comfortable position. I now had a splint sewed into the back of the stays, extending two inches above the blistered part to the bottom of the ribs, by which means the chest and back were supported and the injured part kept at rest, to the patient's great relief. It allowed him to be handled more freely and gave him greater power of motion. The medicine was continued three times daily, and nourishing diet.

Convalescence was now very rapid. He began to move about the house and take his food well. I had not seen him for four days, when I was sent for, as during the night he had got up screaming so loudly as to arouse everybody. It seemed a kind of nightmare; but it had been accompanied with a return of the spasms, as his tongue had been badly bitten. I re-opened the blister by applying an ointment composed of Liq. Epispasticus (Smith's) ♏ xii, Vaseline ʒ ij, which answered well, and his medicine ordered to be given him every night at bedtime. Without further treatment he gradually improved, and in the course of other ten days he was running about. The stays and splint he continued to wear for some time.

The cause of the injury, so far as could be ascertained, was that he had been knocked down by a companion, and had fallen on his back; and such a cause as this is given by Erichsen. In this case the injury was central, so to speak, directly implicating the spinal cord and the nerves controlling the muscles of the jaws and neck. Would tetanus arising from an injury to the extremities have had a more fatal tendency and been less amenable to similar treatment? Six days elapsed between the onset of the tetanus and the application of the blister to the injured part; yet, by the powers of the chloral and bromide of potassium, the spasms were kept in abeyance. When the dose was reduced the spasms returned. Undoubtedly, had we not been able to do so, other twelve hours of the same exhausting agony as he passed on the second night of my attendance would have been fatal to him. I was deterred from trying Calabar bean in this case from the want of success that attended its use in a case reported from the Western Infirmary about the time. The Calabar bean seemed to me to have been too cumulative in its action. While allaying muscular spasm the large quantity required to be given seemed to act fatally on the vital organs, while chloral is easily and rapidly eliminated. This case was an intensely interesting one, and most pleasing in its results.

CURRENT TOPICS.

Glasgow Hospital and Dispensary for the Diseases of the Ear.—The Directors have appointed Thomas Barr, M.D., F.F.P.S., Aural Surgeon and Lecturer on Aural Surgery to the Institution, in place of Dr. J. Patterson Cassells, resigned in bad health; and James Erskine, M.A., M.B., C.M., &c., has been appointed Assistant Aural Surgeon to the above Institution.

CORRESPONDENCE.

A New Effervescing Chalybeate Spring at Strathpeffer Spa, N.B.

(To the Editors of the Glasgow Medical Journal.)

Sirs,—Will you allow me to bring under the notice of your readers a new effervescing chalybeate at this Spa. For centuries this spring has been well known in the Highlands as the "Iron Well." It rises at the foot of Ben Wyvis, about three miles from the Spa, and was brought in iron pipes to the Pump-room a few years ago. Up to the present time the water had stood, on its way to the Pump-room, first, in the large, deep open well where it rises, and then at the Spa buildings in an open filter, allowing of the partial precipitation of the iron at each place from atmospheric oxidation, and the escape of carbonic acid gas, the iron existing in the spring as a carbonate of the protoxide. The filter having now been done away with, and the well emptied down to the effluent pipe at its bottom, the result is the conservation of the iron and of a very large amount of free carbonic acid gas in the water, with the delivery at the now permanently open taps in the Pump-room of an opalescent highly effervescent chalybeate, which, when received in the drinking glass, gradually clears up like champagne.

Like the most used and the most successful iron waters, it contains the essentially small quantity of iron (in its case $\frac{1}{4}$ grain of the carbonate to the 10 ounces), and as the supply of the water is abundant (900 gallons per day), there will be sufficient for carbonated chalybeate baths in addition to the present Sulphur Bath establishment at the Spa. Besides, therefore, Strathpeffer possessing sulphur springs, among the strongest of their class in Europe, as well as recently established facilities for the inhalation of sulphuretted hydrogen vapour, it will now have the advantage of a first class acidulous chalybeate, to enhance its attractions as a Highland health resort.

By mixing the chalybeate with the sulphur water an ink-black combination is the result.—I am, Sirs, yours faithfully,

D. MANSON, M.D.

STRATHPEFFER SPA,
April 11th, 1884.

Obituary.

ALLEN THOMSON, M.D., D.C.L., F.R.S., LL.D.

It is with deep regret that we announce the death of Dr. Allen Thomson, who, for many years, occupied the Chair of Anatomy in the University, and took an active part in all medical matters connected with the College and our large hospitals. He died on Friday, the 21st of March, in the seventy-fifth year of his age.

Dr. Allen Thomson was the son of Dr. John Thomson, who for some time occupied the Chair of Military Surgery, and afterwards the Chair of Pathology, in the University of Edinburgh. He received his medical education and graduated in the University of Edinburgh in 1830, shortly after which he took the Fellowship of the Royal College of Surgeons of Edinburgh, and almost immediately thereafter commenced a course of lectures on Anatomy at the Extra-Mural School, which course of instruction he continued to deliver till he was appointed to the Chair of Anatomy in the University of Aberdeen in 1839. In 1842 he returned to Edinburgh, having received the appointment of Professor of Physiology in the University. This chair he only occupied for six years; but during that time he succeeded in establishing in the University the present system of written and oral examinations in place of the simple and unsatisfactory method of oral examination, which, till that time, was the only test applied to students on presenting themselves for a degree.

In 1848 he was appointed to the Chair of Anatomy in the University of Glasgow, a position which he retained till 1877, when he retired and settled in London, after which he became a member of the Council, and ultimately Vice-President of the Royal Society of London—a Society of which he had been a member from the time he received his appointment to the Chair in the University of this city. Dr. Allen Thomson took an active part in connection with many of our public institutions, but more particularly in the management of our large hospitals. He was for many years on the Board of Managers of the Royal Infirmary, and to his influence and support the origin and development of the now large Pathological Museum of that institution are largely due.

The Western Infirmary, in which he took a special interest, both while it was being built and during its earlier years, owes its existence, to a great extent, to his activity and good judgment; and we know that there were few institutions of the kind in this country to be compared with the hospital which now provides material for clinical instruction to the students of Glasgow University.

He was Chairman, and one of the most active members, of the Removal and Building Committee of the University, which arranged, and ultimately completed, the erection of the New College Buildings, and the removal of the University from High Street to the more healthful and commodious premises at Gilmorehill. In carrying out this extensive and difficult scheme Dr. Thomson had the full confidence of his colleagues and of those interested in the wellbeing of the University, and by all he was considered to be one of the best judges of the requirements of a rapidly growing medical school. But while he made what he and all others at the time considered to be ample provision for increase in the number of students and development of the methods of teaching, it is now abundantly apparent that it is insufficient. Not only have the numbers of students increased beyond all anticipation, but the system of instruction has altered so much that the class rooms, and particularly the laboratories, are not large enough to meet the requirements of the time.

In this *Journal* Dr. Allen Thomson always took great interest. In the year 1869, when the present series of the *Journal* was originated by a joint effort of the leading professional associations of Glasgow, he stood forward as the most prominent representative of the whole profession, and was elected the first President of the Glasgow and West of Scotland Medical Association, founded in order to carry on the *Journal.* This office he retained for a number of years.

Outside this city Dr. Thomson took an active interest in all scientific work. He was President of the Biological Section of the British Association when it met in Edinburgh in 1871; and in 1877 he was elected President of the Association, and in his Presidential address, on "The Development of the Forms of Animal Life," he discussed his views in relation to the theory of evolution in a most interesting and comprehensive manner.

He was the first President of the Glasgow and West of Scotland Branch of the British Medical Association.

Dr. Thomson contributed largely to Medical literature, and the results of his labour may be found in the *Transactions of the Royal Societies of London and of Edinburgh*, in the *Medical Journals*, in *Todd's Cyclopædia of Anatomy*, and in *Quain's Anatomy;* but embryology is, above all others, the subject to which he devoted most labour, and the one with which his name is identified.

Dr. Allen Thomson was an excellent teacher, and possessed a wonderful power of illustrating his ideas by drawings on the black-board. He was clear and careful in his methods of observation and expression, critical of new observations, but still very open to conviction when proof of their truth was supplied. His intellect was highly cultivated; he was judicious in council and clear in judgment; courteous in manner, and always willing to assist those who showed an aptitude for, and willingness to, work. He was loved by many, and respected by all his students and colleagues; and now that he has passed away from us, we can only think of him kindly, and appreciate his good work and influence.

Dr. J. Patterson Cassells.

We have also to record the death of Dr. Cassells, which took place on the 13th April, in the forty-eighth year of his age, after a somewhat prolonged illness. Dr. Cassells was well known to the readers of this *Journal*, to which he contributed frequently on subjects connected with diseases of the ear. His latest work, the translation of Politzer's work on this subject, was favourably noticed in these pages. Dr. Cassells was a hard working and careful practitioner of the specialty to which he devoted himself; and he did much to bring this specialty before the profession in Glasgow. We believe that he was induced to adopt this department of surgery from the peculiar infirmity from which he suffered for many years, and which finally compelled him to give up practice. This he did about a year ago, and he has since then lived in retirement. Dying at a comparatively early age, he has left a record of much careful and useful work behind him.

REVIEWS.

Clinical Lectures on Mental Diseases. By T. S. Clouston, M.D., &c. London: J. & A. Churchill.

In Scotland, what is termed the moral treatment of insanity in asylums has been cultivated more thoroughly, and has reached a higher point of perfection, than in any other European country. It is not to be supposed that in practice this moral treatment is the nice adjustment, by skilled attendants and otherwise, of moral and intellectual methods to the mental and physical necessities of the individual patient. The conditions of modern asylums permit but little, if any, of such specialisation as this would imply. To the uninitiated we may explain that what is meant by this satisfactory kind of phrase is the providing of home-like conditions in asylums. The employment of the patients, and this so far as possible in out-door labour, and the removal of all restrictions on their liberty not demanded in their own or other people's interests. The latest triumph in this direction is the doing away with locked doors in many of our asylums; indeed, the way in which this innovation has been hailed by some raises doubts as to the efficiency of the other methods of treatment in use. It at any rate points to a bias which is certainly not medical, as we commonly understand the word. The common sense of the matter is that much credit is taken for discovering what, but for the defective and unscientific character of the provision we make for the care and cure of the insane, would have been discovered long ago—*i. e.*, that it is not necessary to adjust the surroundings adapted to the care of chronic cases, which form the great bulk of the patients in asylums, to the conditions required for the restraint or safe keeping of the few dangerous cases. The treatment of the acute and curable forms of insanity in institutions such as our present burgh and county asylums, which are essentially homes for the care of idiots, imbeciles, dements, "et hoc genus omne," is a wrong done to these patients, while the effect upon the medical officers of asylums is to make of them, in too many cases, mere asylum managers, having their energies almost entirely devoted to administrative duties. Even the Scotch Board of Lunacy, in one of their annual reports, state very clearly that a knowledge of agriculture is of at least as much importance to asylum physicians as a knowledge of medicine; and in the same Blue Book they point with satisfaction to the history and statistics of the Banff District Asylum, where medical direction is at a minimum—the superintendent being a layman, the spade and barrow the most important therapeutical instruments. Dealing with things as they are, and from their point of view, the Board of Lunacy are not far wrong in their contention. We do not desire less of such excellent things as good food, fresh air, and healthy employment, we only desire a little more clinical work; in short, a little more medicine, in the best sense of the word, rather than a little less farming. The hope for the future, as it has been the best help in the past, is that we take a thoroughly medical view of the nature of insanity, and only in this way may we hope to know more about the nature and treatment of those forms of brain disease which have mental disturbance as their only prominent symptom. And we venture to think that this will be only well done as a general thing when we have really hospitals for the actually insane.

These reflections very naturally arise in connection with the novelty of the appearance of a good book on mental disease viewed from the clinical and practical side, and by so distinguished a physician as Dr. Clouston. This book consists of nineteen lectures, and in these lectures 260 cases of every variety of mental disease are described and embodied. We might take exception to Dr. Clouston's classification, so far as it is an attempt at classification, and particularly to such words as psychalgia, psychoclampsia, psychocoma, which he uses as synonymous with states of mental depression, states of mental exaltation, and states of stupor respectively; but looking at them as merely suggestive helps to the practical consideration of a most obscure subject, rather than as scientific definitions, our objections disappear. We might also object to the occasionally somewhat too dogmatic presentation of opinions on subjects about which there is room for considerable difference of opinion. These lectures are obviously the outcome of large and varied experience and keen insight. They are full of practical hints of great value, both as to the prophylaxis and treatment of mental disease. For the student and general practitioner, it is upon the whole the best book we know of for their purposes. The latter will find in its pages direction and guidance of the most reliable kind for the proper discharge of some of the most delicate and important duties of his profession.

There are a number of plates illustrative of the morbid histology of the brain.

Apart from the practical value of the book to the practitioner, we venture to hope that it will, and it is suggestive enough for the purpose, stimulate the efforts of others in the way of the clinical study of mental disease.

The Diseases of Children: a Hand-book for Practitioners and Students. By ARMAND SEMPLE, B.A., M.B. Cantab, M.R.C.P. London. London: Baillière, Tindall, & Cox. 1884.

THERE are always numerous objections ready to hand for works of the class to which Dr. Semple's book belongs. It is more pretentious than the old-fashioned dictionaries associated with the names of Cooper and Hooper; and yet it cannot be called an encyclopedia—a term which might fairly be applied to Dr. Quain's *Dictionary.* It is not the index to a fuller work, as was that useful little book of Dr. Tanner's, and the most ardent admirer could not call it a full and satisfactory treatise. Its sphere of usefulness, if it has any, is a more modern requirement developed by the increased range of subjects upon which students are examined, and the not increased period of curriculum.

But a compendium to be useful to the student should contain the gist of well authenticated and acknowledged opinions, and should enable him to support his answer with the name of his authority. Dr. Semple's claim to be an authority must rest upon further evidence than this book. A work which, like the one before us, traverses a vast subject, must not be judged by the individual strong or feeble points in its execution, and we have made no attempt to do so; we have rather endeavoured to estimate how far the practitioner will find it a safe resort and a ready help. Did the book belong to a different class we could refer to chapters on Pneumonia or on Idiocy as good, or to chapters on Bronchitis, Asthma, or Heart Diseases as not so. We might refer to a compendium of hints on the clinical examination of children, or on the estimation of brain power, as worth remembering, and we might indicate other things as better forgotten. Such criticism, however, would be second place were the general merits of the work of a superior order. We prefer to say that the work is more distinctly weak in that it fails in many important points to give sufficient prominence to such diseases as are greatly modified in children. We have already indicated that pneumonia is an exception, and, we would add, not the only one. But infantile rheumatism is surely incompletely described, and, strange to say, diphtheria is a very unfinished picture. A glance at the infectious diseases will not help a young practitioner in his advice to parents, guardians, and schoolmasters, although he might, without unreason, turn to such a book for assistance.

It is upon some such ground mainly that we take the liberty to doubt the call for this book.

No student begins his experience of medicine with the study of disease as it is seen in children, and the practitioner is usually first exercised over the diseases of servants. The diseases of children are always a superadded subject, and probably all students regard them as deviations from the normal type. When the tyro has mastered what is commonly regarded as practice of physic he naturally turns his attention to the immortal Lectures of Charles West, and learns how and why disease or certain diseases are modified in children. At the first leisure moment Eustace Smith claims his interest, and as early as possible Meigs and Pepper's work is placed on his book-shelf for frequent reference.

Where, then, is the place for Dr. Semple's book? It is dogmatic, but neither attractive nor convincing. It leans upon nothing previously included in the curriculum, and it is too meagre to be a satisfactory reference or treatise. It does not omit the diseases common to children and their elders, and it does not sufficiently render in detail the diseases which so far differ from the adult type to merit the title of infantile complaints. If a man learned his practice of medicine from an index only (even such an index as Tanner's) he might be content to learn the diseases of children as a new study from such a book as Dr. Semple's. We could scarcely in either case consider him an intelligent practitioner, and an anxious mother would soon teach him his shortcomings.

We have noticed this book in general terms, and we have adduced little evidence in support of our opinion. It must not be surmised that we have formed that opinion on insufficient grounds. We have read its pages carefully, and compared its descriptions with the larger treatises; we have carried it with us in a large practice among children over two months, and referred to it constantly in cases which were tedious, difficult, or unsatisfactory. With much regret we admit that we have not found it a help. Dr. Semple's opportunities and the information indicated rather than contained in his book lead us to hope that some day he will write a more useful work. There is abundant scope for an English author in this direction.

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1. *Transactions of the American Medical Association.* Vol. XXXIII. 1882.
2. *Transactions of the American Surgical Association.* Vol. I. 1883.
3. *Transactions of the College of Physicians of Philadelphia.* Third Series. Vol. VI. 1883.

These handsome volumes indicate clearly the enthusiasm with which the affairs of the various medical institutions of America are conducted. They are not only filled with important scientific matter, but afford interesting glimpses of the inner life of the American Associations. The party warfare, from which they in America are no more exempt than we on this side of the Atlantic, is kept well in the background, however, and the question as to the code of ethics and consultation with "irregulars," round which such a fierce fight was waged last year, and which occupied so much space in the journals at the time, crops up only incidentally, and is not discussed at length. It is satisfactory to note that the inclination of the profession in America is still towards the old paths as defined in their unequalled code of ethics; and that the latitudinarian views of the New York Medical Society met with prompt repudiation by the Association as a whole.

In the first mentioned of these volumes we have many valuable papers. Dr. John V. Shoemaker writes strongly in favour of the subcutaneous injection of mercury in syphilis, and on potassium chlorate in its multifarious applications. Dr. L. Turnbull has also a suggestive paper on a case of diphtheria, in the membrane from which, however, no fungus characteristic of the disease was found. Dr. J. S. Marshall prefers a plea for the appointment of Dental Surgeons in the Army and Navy, and quotes from a letter from Admiral Porter, in which that distinguished veteran says: "If we had had dentists in the Navy, I should not have been compelled to live on soft food to-day." The volume closes with a report on American necrology, and the organisation and code of ethics of the Association.

The American Surgical Association has only recently sprung into existence, and is intended not to injure the old American Medical Association, but to rouse it "from its Rip Van Winkle slumbers and infuse new life into it." It ought to be successful. The first volume of its transactions is in every respect creditable, both as to its get up and the value of its contents. It affords abundant proof of the fact that it is mainly in the direction of surgery that our American brethren are advancing. Not the least valuable of the papers here published are those by the venerable president of the Association, Dr. S. D. Gross, and by Dr. S. W. Gross, on morbid growths.

In the transactions of the Philadelphia College we have much that repays perusal. Papers and discussions on abscess of brain, micrococci in the blood in malignant measles, the bacillus tuberculosis, albuminuria, heart-puncture and heart-suture, management of enteric fever by specific treatment, arsenical paralysis, tubercular cerebro-spinal meningitis, infant foods, relation of pain to weather, &c.

The Asclepiad: a Book of Original Research and Observation. No. 1, Vol. I. By Benjamin Ward Richardson, M.D., F.R.S. London: Eade & Caulfield.

In 1861 Dr. Richardson published a volume of clinical essays under the title *The Asclepiad.* The publication under review is a continuation of the same, issued in quarterly parts instead of in annual volumes. It differs from most periodicals in being entirely from the pen of one writer—its respected editor and author, Dr. Richardson himself.

The most noticeable papers in this number are two: one on "Felicity as a sanitary research," a reprint of a speech delivered in Glasgow in September of last year; and another on "Morphia habitués and their treatment." With the former we are all already familiar. In the latter Dr. Richardson gives us the results of a large experience in the treatment of this class of cases. In treatment he pronounces decidedly against the giving of a substitute, such as alcohol or chloral, after the withdrawal of the morphia. Equally bad, it seems to him, is the sudden withdrawal without substitution; this plan he regards as impracticable, except in those rarely observed cases in which the habit is very recent and the amount of morphia used is small. He recommends "the tapering off plan," the systematic, firm, and gradual withdrawal of the morphia; other aids, such as change of scene, regulation of diet, and tonics, being also employed. The withdrawal should be as rapid as is safe; the time taken should be from 14 to 21 days. "If the quantity were six grains in the twenty-four hours, taken in one-grain doses, I should reduce it by giving five grains, divided into six equal doses, on the first day; four grains, similarly divided, on the second day; three grains on the third and on the fourth days; two grains, again divided into six parts, on the fifth day; one grain on the sixth day, and in like manner as to number of administrations. On the seventh day I should divide half a grain into six doses as before, but should drop one administration; and then day by day continue to drop one administration until there were none."

We commend *The Asclepiad* to the notice of our readers. They will find it full of all that is characteristic of Dr. Richardson's work.

A Few Words upon Anæsthetics. By R. T. Freeman, L.R.C.P., M.R.C.S., Eng. London: J. & A. Churchill. 1883.

Mr. Freeman gives here, in very "few words," a condensed history of anæsthetics, which he wishes to be received as the pioneer to a more exhaustive treatise he has had in preparation for some time. He notices chiefly nitrous oxide, chloroform, ether, and bichloride of methylene, and advocates strongly the practice of beginning anæsthesia with nitrous oxide, continuing with chloroform or ether, as may be deemed advisable. In connection with this view he gives a short account of various apparatuses, by means of which this can be conveniently done, and ends by describing a *Collective Apparatus for administering Anæsthetics,* of his own invention. By using it "the operator has the power of admitting to the face-piece, at a second's notice, either (*a*) nitrous oxide only, (*b*) nitrous oxide and bichloride of methylene or chloroform, (*c*) ether or choloroform only, or (*d*) atmospheric air." The chief advantage claimed for this mode of producing anæsthesia seems to be that it does away with the preliminary struggling, the convulsive movements and consequent asphyxial symptoms, so often caused by chloroform and ether.

This work is a handy little compendium of information on the particular department of the subject it discusses—namely, appliances for the administration of anæsthetics. It is to be regretted that so little of its space is devoted to the physiological action of anæsthetics.

Elements of Practical Medicine. By ALFRED H. CARTER, M.D. London. Second edition. London: H. K. Lewis. 1883.

THE object of this work, in the terms of the preface, is "partly to provide the student with a general introduction to the study of medicine, and partly to bring the essentials of the subject—so far as required for the ordinary medical qualifications—within the grasp of those who are not disposed or have not the leisure to read the large and complete works, a class of readers which, in my opinion, meets with too little sympathy." It seems to us that it has been mainly designed for "those who are *not disposed* to read the large and complete works." As this is a class with which we have little sympathy, it is not to be wondered at that we cannot accord to Dr. Carter much credit for his production. It is a grind book, and can be of use only to a student who has attended lectures or read some large work. It is not elementary enough to be of service to a student commencing the study of the practice of medicine, as he would require to go to a larger work for definitions of many of the terms employed—viz., incubation, inversion, and all the terms applied to the phenomena of auscultation, percussion, and physical examination generally. Besides, it possesses the faults of all such condensations.

Leben & Co.'s Clinical Figures. 23 Southampton Buildings, Chancery Lane, London, W.C.

We have received proofs of these outlines of the brain and of the viscera. Those of the brain are in two sheets, the first containing 6 figures of the external aspects of the brain, and the second containing 12 of various sections, the former being after Ecker and the latter after Pitrés. They are intended to supply physicians with outlines on which to sketch lesions met with, and the artists offer to supply electro blocks to illustrate such special sketches at a cheap rate. We think the outlines of the brain will be useful, but their value would be enhanced by indicating the names of the various parts shown. The outlines of the anterior and posterior aspects of the thoracic and abdominal viscera are too small, so that the figures are too crowded. Besides, we are inclined to doubt the accuracy of the representation.

The Chinese Opium Smoker. Twelve illustrations, facsimiles of native drawings. London: S. W. Partridge & Co.

These coloured drawings, "reproduced from the Chinese," have, for years past, we are told in the preface, "been posted up in the wayside tea houses, and on the walls of public places amongst the Chinese people, at the expense of native gentlemen who have been alarmed at the spread of opium smoking." They have all the Chinese quaintness in conception, colour, and drawing, which stamp them as genuine; and give ample evidence of the fact that there exists in China a party strongly opposed to opium smoking and the opium traffic. The series is extremely cheap, costing sixpence, and merely as specimens of Eastern art should be of interest even to the most rabid advocate of the blessings of the opium traffic.

The South African Medical Journal. Vol. I, Nos. 1 and 2.

We have here the first two numbers of the only medical journal published in Cape Colony, issued (of all places in the world) at East London. Possibly the fact that it appears in this little town may tone down any feeling of jealousy on the part of practitioners in larger towns and in the adjoining colonies. The *Journal* is under able editorship, that of Dr. W. Darley-Hartley, and promises well. We wish it a long and vigorous career.

Vivisection: in its Scientific, Religious, and Moral aspects. By E. D. Girdlestone, B.A. London: Simpkins, Marshall, & Co. 1884.

This is a sensible defence of the sensible side of the "Vivisection" question. To those who are not heartily sick of the whole controversy its perusal may be recommended. The value of this defence is probably enhanced by the fact that it emanates from one who is outside the ranks of the profession; at any rate, it will be the more likely to appeal with power to some minds on this account.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

Reports under the Supervision of J. LINDSAY STEVEN, M.B.

From Professor M'Call Anderson's Wards.

I.—Case of generalised psoriasis cured by the subcutaneous injection of liquor sodæ arseniatis.—[Reported by W. L. Strain, M.B., House Physician.] John M., æt 19, sailor, was admitted into the cutaneous wards of the Western Infirmary on December 19, 1883, suffering from psoriasis of 15 years' duration.

The eruption at the time of his admission was very extensive; scarcely a spot of sound skin was to be found on his whole body. The palms of his hands and soles of his feet were deeply fissured—so much so that he was quite unable to use his hands or to walk any distance. The bend of the elbows and also of the knees was deeply fissured, and any attempt at extension gave rise to great pain. The trunk of his body was covered with large silvery flakes, and the skin underneath was somewhat inflamed. The head and face were badly affected, and for some time he could hardly open his eyes.

During the 15 years he has had the disease it has never been completely away, and at the times when it was best it was confined pretty much to the elbows and knees. He never was so ill before, and this aggravation was of about three weeks' duration.

He has tried various remedies, and has been under many different kinds of treatment, but with very little effect.

When admitted, in addition to regulating his bowels, he was put upon half-drachm doses of pix liquida. This was continued for about six weeks, being pushed to the point of disagreeing, and no improvement resulted, but rather the opposite. On 10th February it was stopped, and on the 13th the hypodermic administration of Liq. Sodæ Arseniatis (B.P.) was commenced in doses of ♏10 daily. It being found to agree quite well, and producing no local discomfort, it was increased to about ♏20 daily, injected into the buttock, the skin having been frozen.

In four days after beginning this treatment patient observed a distinct improvement, nearly all the scales having fallen off his body, leaving patches of slightly inflamed skin. On 12th March he was shown to the clinical class almost entirely free of the eruption, only a few small spots remaining on the front of his legs.

His skin looks quite healthy, and his hands and feet have quite recovered.

About an ounce and a quarter of the solution of the arseniate of soda was given in all, and no unpleasant symptom developed, his general health having all along been very good.

From Dr. Hector C. Cameron's Wards.

II.—SCIATICA TREATED BY NERVE STRETCHING.—[Reported by Alfred Williams, M.B., House Surgeon.] Mary B., æt 48, housewife, admitted November 20, 1883, complaining of severe shooting pain in her left leg.

Last May she experienced the pain for the first time. It commenced during the night, and woke her from sleep. Ever since very few days have passed on which she has been entirely free of pain; it has often been so bad as to confine her to bed. The pain is generally more severe during the night. It begins in the back of the thigh, a little higher than the knee, and from this point it shoots upwards to the hip, and down into her heel.

In October she was admitted to the medical wards, and there treated by friction and local applications. This lessened the pain considerably, but did not entirely relieve her. It was then thought advisable to try if stretching the sciatic nerve would do any good, and she was transferred to the surgical ward. Thereupon her pain became suddenly less—so much so that it was not sufficient to warrant an operation. She was dismissed on 4th December.

On 29th December she again sought admission, begging to have something done to relieve her of pain. She stated that during her absence she had been lying in bed, being unable to get about.

Patient was readmitted, and on 3rd January, having been put under chloroform, an incision was made in the back of the thigh below the gluteal fold. There was some matting of the tissues round the nerve, but it was soon found. A considerable amount of steady traction was then made on it, both in an upward and downward direction.

The wound was stitched and dressed with antiseptic precautions. For the rest of the day she complained of some soreness about the wound; but she slept well during the night, without feeling a twinge of sciatica, and from then up to the present time she has had no return of the pain. The wound is now completely healed, and the patient is walking about the ward. She feels the left leg a little weak and benumbed, but this is improving.

MEETINGS OF SOCIETIES.

GLASGOW SOUTHERN MEDICAL SOCIETY.

Meeting X.—*6th March, 1884.*

The President, Dr. Park, *in the Chair.*

The Society met, by invitation of Dr. Wm. M'Ewen, in the operating theatre of ward 22, in the Glasgow Royal Infirmary, for a clinical demonstration illustrating operative treatment in cerebral and spinal lesions. Fourteen cases were shown—twelve cerebral and two spinal. The cerebral cases, with one exception, were traumatic, and were all treated on the same principles: trephining, elevation where necessary, incision of dura mater or brain, and evacuation of pus or blood. The bone removed by the trephine was in most instances broken up into small pieces and re-implanted, the result usually being osseous union of the fragments to the skull, giving complete protection to the brain as before.

The exception was the case of a girl suffering from a tumour projecting through the superior orbital plate of the left eye in front of the eyeball. She suffered from severe convulsions. The tumour was found between the dura mater and the bone. The dura mater was separated from the bone for a little distance on each side of the tumour, which allowed of the brain being compressed slightly backwards to allow the tumour to be detached by the knife from the dura mater. She made an excellent recovery with very slight deformity.

The spinal cases were treated on the same principles, and resulted in the cure respectively of a paraplegia and a hemiplegia. The cases were conducted antiseptically throughout, and there was rarely the formation of pus or any rise in temperature.

Meeting XI.—*20th March, 1884.*

The President, Dr. Park, *in the Chair.*

Dr. Hector C. Cameron was unanimously elected a member.

The President opened a professional conversation on the subject of last night's demonstration, referring more particularly to the science of cerebral localisation and the circumstances that might call for surgical interference in cases happening in ordinary medical practice. He showed a patient manifesting a cerebral lesion, and showing the difficulty which general practitioners had in determining when to call in surgical aid.

Mr. M'Millan was astonished at the results of surgical interference in injuries and disease of the head and spinal cord, and complimented Dr. M'Ewen highly on his skill. The subject was one which required great study to undertake and great care in the work.

Mr. Stuart Nairne said that it was comparatively easy to determine what to do in cases of injury if one only determined to do it. But it was in ordinary medical cases that the great difficulty would be found in determining what to do. While acknowledging the good work of Charcot, Ferrier, and others, he had still sufficient belief in Brown-Séquard to deter him from approving of any surgical interference on the brain in a case that might be called part medical—*i.e.*, when there was nothing but subjective phenomena to go on. It was different with the spinal cord where the mapping out had nearly reached perfection. He thought the President's case one of hysteria, probably depending on some menstrual derangement.

Mr. T. F. Gilmour thought Dr. M'Ewen's cases worthy of all praise, and that general practitioners ought to be more alive to the various cerebral and spinal cases they see, endeavour to localise the lesions, and when medicine fails not to delay too long in applying surgical interference. He narrated the case of a man with intense vertical cephalalgia. He treated him with iodide of potass with no benefit for some time, and at last applied a fly blister on the vertex. The blister rose, and the patient at the same time got deeply intoxicated; and when he became sober the pain was gone and had never returned. He thought Dr. Park's case represented either hysteria or syphilis.

Dr. Park still maintained his opinion that his patient was suffering from some cerebral lesion in an early stage. He had given her iodide of potassium with undoubted benefit; but the symptoms he referred to pointed to cerebral lesion, weak grasp of the hand, inequality of the pupils, and a painful spot over the vertex, best found by Dr. Robertson's method of tapping. He thought Dr. Gilmour's case one of limited meningitis, cured by counter-irritation.

Dr. M'Ewen said he was pleased that his demonstration had evoked so much interest; and he was sure it would be very much better if cerebral and spinal cases were treated surgically earlier than they have been hitherto done. He was doubtful of Brown-Séquard's cases; he would like to have seen them. But, at the same time, he was willing to grant that there was a great deal of obscurity still about the localisation of brain functions, and it could only be in cases of undoubted diagnosis that an operation would be justifiable. He recommended Charcot's and Hitzig's maps for the study of localisation.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Physiological Albuminuria: Secretion of Urine.—The following paragraphs, both taken from the *Cbl. f. d. Med. Wiss.*, 19th April, 1884, should be read with interest in connection with the recent discussion on albuminuria at the Pathological and Clinical Society:—

1. M. de Chateaubourg, in a monograph published in Paris in 1883, gives the results of some investigations on the so-called "physiological albuminuria." He found that albuminuria is extremely common among healthy persons. It was detected in 592 out of 701 persons examined—that is, in 84 per cent. It was found that the influence of exercise in this matter was of great importance: thus, of 120 infantry soldiers examined early in the morning, in 92 there was albumen discovered in the urine—that is, in 76 per cent; after a fatiguing march the percentage rose to 90, while the quantity of albumen also increased. Severe mental labour was also found to increase this physiological albuminuria: thus, in the urine of 50 young people preparing for an examination albumen was found in 46 instances—that is, in 92 per cent; and in considerable quantity. Most marked of all was the influence of the cold bath: thus, of 53 soldiers fasting, but not suffering from fatigue, after a cold bath of five minutes' duration every one gave evidence of the presence of albumen in the urine. Contrary to some other investigators (Senator, Rendall, &c.), the author did not observe that food affected the albuminuria in any marked degree. Further, of 142 children, varying in age from 6 to 14, albuminuria was discovered in 111 (78 per cent), but usually in minuter traces than in adults. [No reference is made in this paragraph to the mode of testing adopted.]

2. Dr. M. Abeles removed the kidneys of freshly killed dogs and passed through them, under a pressure of 130 mm. mercury, later of 170 mm. mercury, a current of defibrinated blood, and analysed the fluid obtained from the ureters. The solution injected consisted of two parts defibrinated blood and one part of a solution which contained 0·6 per cent sodium chloride and $\frac{1}{20000}$th of caustic soda. In the first experiments urea was added to the sodium chloride solution in such proportion that, with the urea of the blood, the cir-

culating fluid contained 0·2 per cent urea. The circulation through the kidneys was kept up for 2–3 hours, from 6 to 8 litres of fluid being used. The fluid dropping from cannulæ inserted into the ureters was taken in three portions, all of which were neutral, all slightly reddish in tinge (containing a few red corpuscles), and albuminous. In the second portion the urea was estimated, and found present to the extent of 0·32 per cent; it thus contained about a half more urea than the fluid circulating in the kidney. When, besides urea, urine sugar was added to the fluid in such amount that, together with that found normally in the blood, the percentage of 0·25 was reached, the secretion caught contained first 0·6, subsequently 0·5, per cent of sugar—that is, double the quantity present in the circulating fluid. The secretion contained no uric acid; it was concentrated also, as compared with the circulating blood. When a fluid containing no urea was introduced no secretion took place; the addition of urea at once accelerated the circulation and started the secretion—facts which support Heidenhain's view that urea is the specific stimulus of the secreting cells of the kidney. The principal result of these experiments is that they show that the kidney treated in the above fashion yields a secretion resembling urine, in which crystalloids (urea, sugar) occur in larger quantity than in the fluid caused to circulate in the renal vessels.—*Wiener akad. Stzgsber.* lxxvii. 3, p. 187.

Transitory Albuminuria.—The following extracts are taken from the *Cbl. f. d. Med. Wiss.*, 15th September, 1883:—

1. Dr. O. Rosenbach refers (*Zeitschr. f. Klin. Med.*, vi, p. 240) to several patients who suffered from severe periodical headache, and whose urine was increased in quantity, extremely acid, of sp. gr. 1012–1015, and distinctly albuminous (without casts, &c.), during, and for a short time after, the attack of headache. Left ventricle hypertrophied, and arterial tension increased; diagnosis, contracted kidney. The most noticeable point was that immediately the headache ceased, or shortly afterwards, the albumen disappeared entirely from the urine. In three other patients, men of middle age, stout, and suffering from fatty enlargement of the liver, hypertrophy of left ventricle, and polyuria, the urine was clear, acid, of sp. gr. 1010–1015, albuminous, but without casts, &c.; diagnosis, contracted kidney. Digitalis had no effect on the albuminuria; but during very considerable intervals albumen *disappeared* entirely from the urine. One of the patients died dropsical. Rosenbach adds certain reflections on the nature and origin

of the contracted kidney, according to which the essential symptoms of the affection are not due to organic or functional disorder of the kidneys. With special reference to the last mentioned cases he connects it with a primary cardiac hypertrophy, due to over-feeding in plethoric individuals, and regards the discharge of albumen, not as the result of renal disease, but as the effect of the action of the kidneys in regulating the amount of albumen contained in the blood; the contraction of the kidney is therefore an atrophy of an organ which has been kept for a time in a state of increased activity. The author adds some cases of transitory albuminuria without evident organic disease. In several of these the albumen in the urine was due to retention of pus in some part of the body, and the patient was free of fever; on discharge of the pus the albuminuria disappeared. Figurate elements were not found in the urine. The albumen was here absorbed from the accumulation of pus, and the albuminuria was the expression of a compensatory action on the part of the kidneys. In a number of cases of young persons, varying in age from 12–16 years, anæmic looking, and suffering from various digestive disorders, the urine was normal, with the exception that it contained albumen, which, however, presented periods of intermission, often a week in duration, and ultimately disappeared. Here again the albuminuria is considered as the manifestation of a self-regulating power of the organism which relieves the blood when it is overloaded with albumen or when it is from any cause unable to enter into combination with the absorbed albumen.

2. Dr. St. M. Rendall (*Thèse de Paris*, 1883) also believes in the hæmatogenous origin of many cases of albuminuria, and emphasises specially the influence of diet in producing this symptom. The cases he records are chiefly those of young persons, anæmic, but not suffering from œdema or any organic disease, in whom an intermittent albuminuria showed itself shortly after meals. Urine, in other respects, normal, except that oxalate of lime crystals were found in large quantity. Perspiration increased. The symptoms are referred solely to transient changes in the composition of the blood plasma, dependent on the periodical appearance of certain albuminous bodies in the blood, this being due to some hitherto unrecognised disturbance of digestion or assimilation, and followed by the discharge of these albuminous bodies through the kidneys, which remain intact. Prognosis in such cases is favourable.

3. Dr. L. Capitan (*Recherches expérimentales et cliniques*

sur les albuminuries transitoires. Paris: 1883) gives special prominence to the transient albuminuria which may be called forth by irritation of various parts of the nervous system (the brain, and not only the well known spot on the floor of the fourth ventricle, but also various points on the surface in rabbits, the spinal cord, peripheral nerves, particularly the sciatic, the organs of special sense), and which he believes due to reflex irritation of the vasomotor nerves of the kidneys. Irritation of the skin, a low temperature, and asphyxia produced albuminuria, which the author ascribed partly to the above-mentioned nervous irritation, and partly to carbonic acid poisoning. As for the albuminuria of healthy persons, the author has studied it in 100 soldiers (varying in age from 21–25 years) and 97 young persons of 1½–18 years, and is of opinion that we have here to do solely with a temporary exaggeration of a probably normal phenomenon, which usually escapes detection only on account of the imperfection of our methods of examination. [Almost exactly the same views were expressed by Professor H. Senator, of Berlin, in a work published by him in 1882, entitled *Albuminuria in healthy and diseased conditions*, of which an interesting *résumé* will be found in *Cbl. f. d. Med. Wiss.*, 1882, p. 441. The paper by Dr. Dukes on *The albuminuria of adolescents* may also be referred to in *Brit. Med. Journal*, 1878.]

On the Permanence of Bacilli in Phthisical Sputa. —Vignal; communicated to the Société de Biologie, 15th December, 1883. The author has, in order to see what becomes of the bacilli in the phthisical sputa, which are daily expectorated on the streets, collected such sputa, allowed them to dry on a plate, moistened them again, then dried them once more, and so on. He found the bacilli in them quite as numerous and intact as in freshly expectorated sputa. He made injection experiments in two guinea pigs: one died in a few days of obstruction of the bowels, and had to be set aside; the second became during the first month fat—an evidence of its good hygienic and alimentary surroundings—then it emaciated, and died after three months. At the *post mortem* numerous tubercles were found in all the organs, with bacilli. These experiments show that the sputa, discharged on the street and the floors of houses, are by no means innocuous, especially to predisposed individuals. —*Deutsche Medizinal Zeitung.* No. 1. 3rd January, 1884. —J. L. S.

bably the largest and heaviest stone on record was removed from the human bladder, in which the patient recovered.

Case.—J. B., a retired draper, was born in the year 1828, and in his youth was a factory operative. In 1845, when at the age of 17, he fell down the well of a hoist, alighting with his legs astride an iron bar, sustaining a double fracture of the left leg, also rupture of the urethra, and laceration of the perinæum. Through the opening in the perinæum, as well as by the urethra, blood passed for a period of two weeks. Two attempts at closure of the perineal fistula were ineffectually made, and the urine continued to dribble without ceasing, compelling B. to resort to padding his clothing for the purpose of absorbing the constantly escaping urine. Several medical men saw him, but as patient was much averse to any operative procedure, nothing was attempted, although patient was aware from the year 1852 of the presence of a calculus.

On the 14th June, 1872, Dr. G. W. was called to see patient, who was then complaining of pain in the bladder, for the relief of which sedatives were prescribed. On the 25th of the same month he was seen again, when nothing abnormal was noticeable beyond the sinus in the perinæum.

On the 9th of the following month (July), Dr. W. was sent for again, when the sinus was seen to be considerably enlarged, and the patient requested the doctor to introduce his finger through the fistula, and he would feel the stone. On passing his finger through the opening, the doctor at once felt the calculus, but his finger entered a large irregular cavity in the stone, and the patient explained the presence of this by saying that he had introduced a chisel, with which he attempted to break up the mass, and had managed in this way to remove about *one ounce*.

The doctor started home for forceps, with which to remove the stone, but during his absence, whilst B. was walking about his room in great pain, the stone suddenly burst the perinæum, and fell heavily on the floor, breaking into two pieces. When expelled it weighed nearly 14 ounces and a half, which, taken with the portion removed by the chisel, makes 15 ounces and a half, which is, so far as I can discover, about the heaviest stone on record in the annals of surgery, in which the patient recovered after its removal. The stone measured in long circumference $10\frac{5}{8}$ in., and in the short circumference $8\frac{1}{8}$ inches.

The lacerated perinæum was dressed with carbolic oil; and on the 15th July, six days from the date of expulsion of the stone, B. was going about collecting his rents and performing his other duties. During the last two years of his life Dr. D. saw him occasionally. He passed his water with difficulty, and was fully convinced that there was another stone in his bladder. To corroborate this opinion, Dr. D. attempted to pass an instrument, but was unable to pass even a No. 1 into the bladder through either urethra or sinus. He died in December, 1883, at the age of 55, of an apoplectic attack, eleven years after the passage of the stone. In addition to the sinus, which never entirely closed, a cicatrix, three inches in length, existed in the perinæum.

Scattered through the older surgical works are to be found some cases where the stone has been found of great size, yet, as showing how uncommon they are, I may refer to the wonderful table given in J. G. Cross's prize essay, written in 1833, where a most accurate detail is furnished of sixty years' lithotomy operations performed in the Norfolk and Norwich hospital. The cases in all amounted to 704, and of that large number there were only 9 stones above 4 ounces in weight, and only two above 6 ounces, and in the last the patients succumbed. There was not a single lithotomy in which the stone weighed 7 ounces in the entire sixty years' operations; in fact, on the whole, there are only a few instances on record of a successful result where the stone exceeded 7 or 8 ounces.

Large stones removed, where patient recovered.—There may have been larger, but the weightiest I can find an account of is one of 15 ounces, and $4\frac{3}{4} \times 3\frac{1}{2}$ inches in diameter, removed by Mr. Harmer, of Norwich.

In 1818, Mr. Charles Mayo, of Winchester, removed by lateral operation a calculus weighing 14 ounces and 2 drachms, and measuring $8\frac{1}{2}$ inches in its smallest circumference, and rather more than 10 inches in its longest.

Klein removed a stone weighing ℥xiij and gr. xxx, $3\frac{1}{2}$ inches in diameter, and 8 inches in circumference.

Cheselden cut out one weighing ℥xij, one ℥x, and one ℥vij successfully.

John Collot, mentioned by Ambrose Paré, removed in the year 1570 a stone weighing ℥ix, and $3\frac{1}{2}$ inches in diameter.

Large stones removed, or where an attempt was made at removal, in which the results were fatal.—In London, in 1809, an attempt was made at removing a stone, when the operator had to leave his task unfinished. The stone weighed 44 ounces, and was 16 inches in circumference. The calculus is, I think, preserved in the Hunterian Museum.

Duguise removed, by the supra-pubic operation, a stone weighing 31 ounces, from a patient aged 65, who lived for six days.

Frere Côme removed from a patient in la Charité, by the high operation, a stone 24 ounces in weight; patient died on the following day.

Hildano mentions a case, operated on by Vitellius, where the man died under the operation; the stone weighed 22 ounces.

Cheselden extracted one weighing 18½ ounces, with a circumference in the long axis 11¼ inches, and in the short axis 10 inches; patient dying next morning.

Sir Astley Cooper cut out a calculus weighing 16 ounces; patient lived only four hours.

Mr. Birch, at St. Thomas's, one of 16 ounces, but patient did not survive.

Large calculi found in the dead body.—Kesselring states that he saw at M. Moraud's, a specimen weighing 6 pounds, 3 ounces.

Tolet gives three examples weighing 50, 32, and 28 ounces. One, taken from the body of a monk in Paris, was 51 ounces in weight. Verduc gives an instance of one 3 pounds, 3 ounces. One was found as large as the head of a new born child in the bladder of a boy at Bury St. Edmonds. An example, 35 ounces in weight, was removed from the body of one Francis Dogood at Aberdeen; and Greenfield mentions cases of 18, 19½, 25½, and 32 ounces. This last was said to be "exceeding hard, compact, triangular, of the colour of flint, and which, with a steel, would strike fire like a flint." The stone, weighing 25½ ounces, was taken from the body of Sir Thomas Adams, Lord Mayor of London, who died at the age of 82. Of Sir Thomas, Greenfield says, "This gentleman was so abstemious and temperate in drinking, that for forty or fifty years, even at public feasts, he never exceeded a pint of French wine for his share, being always used to conclude with a glass of canary. In his ordinary way of living, he drank constantly every morning a large draught of small beer with sugar"!

The stone, of which he gives a plate, filled the entire bladder, and of which he says, "What was most observable in this stone was, that where the ureters were inserted into the bladder, the urine had made itself a *gutter* of a semicircular form, and from the middle of *that* another straight one, through which it fell into *the urethra*."

Here is a remarkable statement by the same old authority, regarding a matter supposed by many to be a recent discovery—"It is scarce credible," says Greenfield, "how much the urethra may be *dilated* both in men and women. I have taken away, from a virgin of sixteen years of age, a triangular stone of the bigness of an egg, if we may imagine it of that shape, by only *dilating* the *urethra*, without cutting or in the least tearing, and by applying ordinary means; in three weeks she was sound and firm, holding her water as well as ever."

To my friend Dr. Dunbar, of Blackburn, Lancashire, I am indebted for the privilege of exhibiting this rare specimen to the Society.

A CONTRIBUTION TO THE DISCUSSION ON ALBUMINURIA.

By ROBERT SAUNDBY, M.D. Edin.

Having been unfortunately unable to take part in the admirably organised discussion at the Glasgow Pathological and Clinical Society, I am much indebted to the courtesy of the editors of this *Journal* for this opportunity of expressing my opinion on certain points which have been raised, and in which I take very great interest.

The diagnostic value of albuminuria.—It has been suggested by Leube,* that albuminuria may depend in certain cases upon an increase, within physiological limits, of the normal porosity of the epithelial covering of the glomeruli; and he † and others have asserted that albuminuria not uncommonly occurs in persons who do not present any other recognisable departure from health; finally, many observers ‡ have recorded examples of albuminuria in individuals who presented no other evidence of renal disease, or any local disorder which could afford a reasonable explanation of the symptom, although it is admitted that they were suffering from more or less derangement of health.

The question that seems to me to concern us practically, is not whether there may be such a physiological albuminuria as Leube believes in, or even whether or not the individuals asserted to be healthy by very competent observers had somewhere concealed about their persons a larger or smaller constitutional flaw, which would not have escaped the notice of more sceptical and subtle persons — a point which can

* *Lehre vom Harn*, p. 372.

† *Virchow's Archiv.* Band lxxii. Heft ii.

‡ Christison, Brunton, Moxon, Rooke, Dukes, Mahomed, Fürbringer, Kinnicutt, myself, and others.

never be satisfactorily solved—but what is the diagnostic and prognostic value of albuminuria as a guide to the organic changes in the kidneys?

I think it must be admitted that the teaching of Bright, and of those who followed him, and crystallised the doctrines which Bright had held in a more plastic form; those sonorous periods of Latham, written more for sound than sense; the too dogmatic teaching which is often thought to be the only way of instilling any ideas into the head of the average medical student; that these have created a deeply rooted and widely spread belief throughout the profession, which only shows its kinship with ordinary human nature by the satisfaction with which it accepts and sticks to, a well expressed formula; that albuminuria, unless it can be accounted for by the admixture of blood or pus with the urine, indicates Bright's disease.

Even among those who do not hold this extreme view, probably a large and increasing number, there are many who hold, with Dr. Johnson,* that "even the smallest trace of albumen in the urine is always pathological," and that albuminuria indicates a condition of renal stress, which sooner or later leads to organic disease.

But there are some who venture to say that neither of these positions has been proved, and that the latter of the two is so important that we must not lightly reject any evidence which bears upon it, and, basing their opinion upon a large body of facts, believe that the significance of albuminuria is by no means unequivocal.

We all admit, I imagine, that there are cases of latent Bright's disease, cases which serve so admirably to adorn a tale, the moral of which is, "don't try to live without a doctor;" but no multiplication of instances can advance the proof that such cases as Dr. Moxon, for example, has described, eventually develop Bright's disease.

Dr. Moxon says that all his cases ended in complete recovery. Some of mine certainly recovered, and are now strong and hearty, with no albuminuria; and others, when last seen, were *in statu quo*, but most have been lost sight of. I often see a former Rugby boy in whom, five years ago, Dr. Clement Dukes found albuminuria, and it may interest him to know that he looks the picture of health, and is a hearty, fresh coloured athletic young man, devoted to all kinds of physical exercises.

Perhaps the strongest case I can give is that of a medical

* "Latent Albuminuria." *Brit. Med. Journal*, 13th December, 1879.

friend now in good health, and certainly free from albuminuria or other indication of renal disease, who seventeen years ago accidentally found albumen in his own urine. He was at that time about 16, and supposed to be in ordinary health; he was a pupil to a country surgeon. This discovery caused him much anxiety, as his master and a physician in the neighbouring town agreed that he was suffering from Bright's disease. The quantity of albumen was large, from 1-8th to 1-4th of a column, and always at its maximum after breakfast, disappearing as the day advanced. After it had existed eighteen months, during which time all sorts of treatment were tried without success, he discovered that he could prevent its occurrence by limiting his breakfast to bread and milk. He thinks the tendency persisted for about another year. In course of time he got confidence, and also got tired of troubling about it; but he occasionally examined his urine, and he is sure that he has found no albumen on any occasion during the last eleven years. I should add that at no time were any casts to be found.

I believe that this was just such a case as those which have recently attracted attention, that my friend never had Bright's disease, and that that diagnosis was an unfortunate mistake which, but for his own discovery of the means by which he could control the symptom, would have interfered seriously with his prospects in life, as his father properly hesitated to incur the expense of giving him a medical education, if he was the subject of a fatal organic disease.

This is not the time or place to discuss the prognosis of Bright's disease; but I should like to express the opinion that it is high time the text books extended the period of "6 months to 2 years," which their authors appear to regard as the average if not the extreme duration of Bright's disease.

Such evidence as I have given, and the facts which probably all practitioners beyond middle life can call to mind, tend to alleviate the prospects of our albuminuric patients, and warrant our taking a more hopeful view of those in whom the albuminuria is unaccompanied by any other evidence of Bright's disease.

The position taken by some of the speakers on this question is quite incomprehensible. If these cases have not Bright's disease, it appears to me that the existence of dyspepsia, lassitude, occasional frontal headache, constipation, or even anæmia, does not constitute a satisfactory explanation, nor does the bringing of the albuminuria by these means into the category of a pathological occurrence advance the solution of the question one whit. In my own experience I have never yet met with a case of albuminuria in a perfectly healthy person, but I have met with many in which I had the strongest reasons for believing that it was not dependent upon any structural disease of the kidneys. Dr. Perry will find, if he will do me the honour to look at my paper,* that my patients are all tabulated under the diseases which they presented, and that the word healthy, applied to some of them, plainly refers to their freedom from *organic* disease.

It is this *absence of organic disease* that is important, yet speakers contented themselves with carping at the use of the words "physiological albuminuria," and appeared to think that the existence of a little anæmia or dyspepsia satisfactorily accounts for everything. If it does, then of course the significance of albuminuria at once disappears; but so far as I understand them, these very speakers are those who attach the greatest importance to it.

But it may be very properly asked, How are we to know when a case of albuminuria is temporary or functional, and when dependent upon organic disease? If we cannot distinguish, is it not prudent to regard all as serious cases till they prove themselves not to be so?

In the microscopic examination of the urine I believe we possess a criterion of the utmost value, which enables us absolutely to distinguish these cases. In the case of a person with albuminuria as the sole indication of renal disease, if the urine is free from casts, we may safely pronounce the absence of Bright's disease; of course, before doing so, the search for casts should have been thoroughly made. If one or two slender hyaline casts alone reward our search, I do not condemn the kidneys, it is a case for delay before pronouncing an opinion; but if they are numerous, and not to be attributed to any temporary cause, we are no longer justified in regarding the kidneys as sound, and the character and number of the casts correspond roughly to the amount and nature of the renal lesion. I have elsewhere † expressed myself fully on this matter, and have described the best methods of searching for casts.

This means affords us a way out of the difficulty. If we do not adopt it, we are without landmarks—no argument, truly, if it is a false means; but, as I believe it to be a sound and safe means, it is incumbent upon us to make use of it. Dr. Greenfield tries to get out of the difficulty by throwing doubts upon the alleged frequency of these cases, but the practical difficulty

* *Brit. Med. Journal*, 1879, vol. ii, p. 699.
† *Birm. Med. Review.* Vol. xiv, p. 103.

is the same whatever their relative numbers. Such cases may be more or less common, but when met with, we have to deal with them, and upon our right understanding of them much, including our own credit, often depends.

I have therefore arrived at the conclusion that albuminuria is not a trustworthy guide to the state of the kidneys.

Tests for albumen.—Such an opinion becomes all the more emphatic if certain tests, hitherto restricted to the chemical laboratory, are to be employed, as is suggested, by the busy practitioner at the bedside in the course of his daily rounds.

I have ventured to define albuminuria as "the presence in the urine of a substance which is coagulable by heat, or precipitated by neutralisation."* This definition includes serum albumen and globulin, while it excludes a variety of albuminous substances which are sometimes present in the urine, but concerning whose pathological relationships we know very little. I venture to think that there has been no advance in our knowledge since that definition was written which warrants our departure from it.

We should not forget that our clinical methods are not scientific experiments, but that they are practical inquiries upon the answers to which we seek to base advice, often affecting most gravely the lives, happiness, and material welfare of our patients. It is not a light thing to tell a man to give up his business or his studies, and winter in Italy, yet I have known this done upon the sole basis of the discovery of albuminuria.

It is, therefore, of the greatest importance that the tests we employ should be free from fallacy, and that methods should not be recommended to the use of busy men for practical purposes, until they have been thoroughly investigated and their sources of fallacy eliminated.

This caution will especially commend itself to those who differ from me by attributing a more serious diagnostic significance to albuminuria, for I should have to ask them to explain the discovery by M. Chateaubourg of albumen in 592 instances out of 701 examinations of the urine of *healthy* persons! This result was obtained with potassio-mercuric iodide, now supplied on blotting paper for the use of busy practitioners. In the existing state of opinion as to the diagnostic value of albuminuria, if such tests become popular, I pity the public. The only advantage that I can foresee will be the immense accession of income which will fall to those whose practice is especially derived from the real or supposed subjects of kidney disorders.

* *Birmingham Med. Review.* Vol. viii, p. 210.

If we adhere to the definition I have given of albuminuria, we shall avoid these pitfalls, and we shall continue to regard boiling and acidulation as the best test for the discovery of albumen in the urine; I am very pleased to observe the strong support given to this test by the remarks of Dr. Roberts.

I propose to go over a few of the principal tests *seriatim*, making such comments as are suggested by some experiments which I have recently undertaken.

The Heat Test.—In order to obtain the best results with this means, the following method and precautions must be adhered to. The urine should be that passed after breakfast, it must be clear, and, if necessary, should be filtered, with or without previous addition of sodium chloride, or magnesium sulphate. Putrid urine is unfit for accurate examination. Fill a test tube two-thirds full of urine, and boil the upper half. *It must be well boiled, not merely heated to boiling point.* Acidulate with a few drops of *dilute* acetic acid. Hold the tube against a shaded background, with the light falling from above, when the faintest haze may be detected by contrast with the clear fluid below. In a bad light, or by artificial light, the detection of a faint haze with certainty is impossible. Practice goes for something, but good light and good eyes are indispensable, and those who have not succeeded in finding albumen so readily by this or other means, as has been recorded by others, should ask themselves whether their enquiries have always been assisted by these indispensable requisites.

The cloud so obtained, if the urine has been filtered after saturation with sodium chloride or magnesium sulphate, is almost certainly serum albumen; in the other case globulin may be present, generally together with serum albumen. We know nothing about the pathological relationships of globulin when it occurs by itself, if it ever does. Estelle has published some cases, and I have met with one instance in which this apparently was so; the possibility of this, as well as of other rare sources of fallacy, should prevent our attributing an over great importance to the discovery of albuminuria *per se*.

Dr. Oliver quotes M. Méhu for the statement that urine, saturated with oxalate of lime, gives, even after filtering, a cloud on boiling which is not dissipated by the addition of acetic acid. I find on experiment that urine, artificially saturated with oxalate of lime, behaves as described; but, on looking over my case book, I find that cases of excessive oxaluria, of which I have several well marked examples, have not presented this peculiarity, or, in other words, their urine has not given a cloud on heating. I am therefore inclined to think that this is not a clinical source of fallacy, but it may be borne in mind. Its importance is very small, as, even under the artificial conditions I employed, the cloud was of the faintest description, so faint, indeed, that I doubt whether I ever diagnosed albuminuria from so faint a haze.

Dr. Kirk asserts that urine, containing much mucin but no albumen, gives a cloud resembling albumen on heating. I have endeavoured to confirm this assertion, using urinary mucin only, and have failed to do so. The turbidity due to the added mucin did not increase on boiling, while on adding dilute acetic acid the mucin coagulated in its characteristic manner, which I maintain is quite unlike the behaviour of albumen, and does not constitute a source of fallacy. Any one wishing to prove this can easily do so by allowing the mucous cloud in healthy non-albuminous urine to settle. After pouring away the clear supernatant fluid, fill a test tube two-thirds full of the turbid fluid, and proceed to test as for albumen in the manner previously described, contrasting what takes place with the corresponding reactions of slightly albuminous urine.

Picric Acid.—This test, when used as Dr. Kirk suggests, by the contact method is very delicate; but, besides giving a cloud with albumen, it also gives a similar reaction with peptone and alkaloids, *e.g.*, quinine, and also with mucin and urates. By heating, the cloud disappears if due to peptone, alkaloids, or urates; but, if phosphates are present in the urine, a second cloud may form which may mask the disappearance of the former one. The fallacy from mucin can only be avoided by testing for mucin by other methods, *e.g.*, citric acid, and comparing the result; this has been described by Dr. Oliver and Dr. Kirk.

To carry out the test by the contact method requires some care and neatness, as the solution is very diffusible. Picric acid used, as is suggested, by the bedside when heat is not procurable, can only prove a fruitful source of error.

Potassio-mercuric Iodide.—This is the test already alluded to as having furnished M. Chateaubourg with 592 examples of albuminuria out of 701 examinations of the urine of *healthy* persons! As a means of detecting serum albumen, it has proved in my hands inferior to boiling or picric acid, while, like the latter, it throws down peptones, but, unlike it these are not dissolved on the application of heat.

Tungstate of Soda.—This substance, with or without citric acid, precipitates peptones as well as albumen, the cloud in both cases being unaffected by heat. It gives also, and especially with citric acid, a marked mucin reaction.

I hope the good sense of the profession will keep it from the folly of adopting either of the last two tests, which are absolutely unfit for clinical use.

The use of picric acid *with heat* is not open to objection; and, as there are individual tastes and prepossessions, it will doubtless be adopted by many. Yet, in my opinion, there can be little question that, for simplicity of manipulation and means, and for delicacy and freedom from fallacy, the test by boiling and acidulation, as I have described it, should be generally adopted by the profession for ordinary clinical purposes.

NOTE OF A CURIOUS INSTANCE OF ABNORMAL DEVELOPMENT OF ADVENTITIOUS FINGERS AND TOES, AS ILLUSTRATING THE INFLUENCE OF HEREDITY IN FIVE CONSECUTIVE GENERATIONS.

By J. S. MUIR, M.B., Glasgow.

(*With a Lithograph.*)

Informant—Mr. M.

Generation I.—*Mr. M.'s own family*, consisting of eight members, five living, three dead.

1. James (dead), not deformed.
2. John (dead), deformed: had six toes on each foot—the additional ones being small toes.
3. Elizabeth (alive), not deformed.
4. Jane (alive), not deformed.
5. Agnes (dead), not deformed.
6. Fanny (alive), deformed in both hands, and in one foot.

A. *Left Hand.*—Has an additional little finger. This additional digit projects from the ulnar side of fifth metacarpal bone at an angle of about 45°. From the web to the tip it measures about half an inch long, is slightly incurved, and resembles in appearance a small thumb springing from the hypothenar eminence. It can only be moved in conjunction with the little finger. See fig. 2.

Fig 1


Fig. 2.


Fig 3.

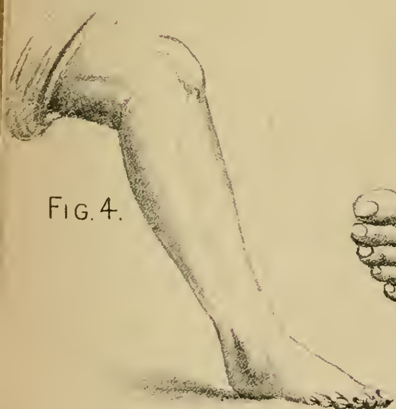
Fig. 4.

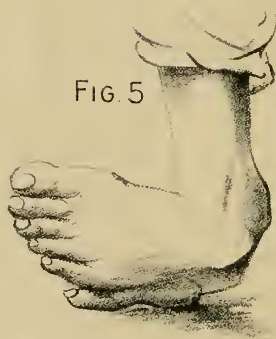
Fig. 5

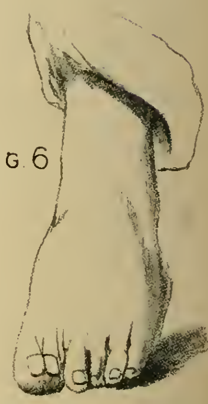
Fig. 6

Fig 7.

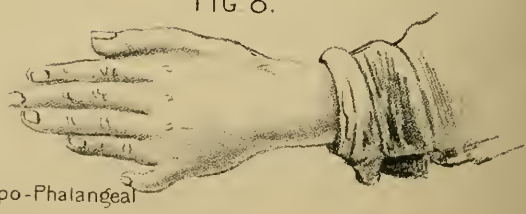
Fig 8.

The fifth metacarpal bone (see diagram of anatomical condition, fig. 1) seems to bifurcate near its proximal extremity into two separate limbs, one of which—the longer—corresponds with the ordinary metacarpal bone of the little finger, which, in other respects, it seems to resemble exactly, while the adventitious limb is shorter, and supports at its distal end the supernumerary finger by a distinct articulation.

B. *Right Hand.*—Has a small abortive finger about $\frac{1}{4}$ of an inch in length, projecting from near the fifth metacarpo-phalangeal joint. It simply hangs loosely by a mere tag of integument, and without any bony connection. It is incapable of any muscular movement, and is terminated by a nail. See fig. 3.

C. *Right Foot.*—Has six complete and separate toes, the accidental one being a small one. See fig. 4.

D. *Left Foot.*—Normal.

7. Robert (alive), Deformity of both hands and both feet.

A. *Right Foot.* — Has an additional small toe which is united by integument to the adjacent small toe, quite up to the nail. The individual phalanges, however, remain quite separate.

B. *Left Foot.*—Presents a curious condition. Has six toes which are all separate, but in addition there is well marked *talipes varus*, the adventitious toe having its plantar surface turned so as to look outwards (anatomically), thus being brought into contact with the ground, and forming part of the surface used in walking. See fig. 5.

C. *Left Hand.*—Resembles very closely Fanny's left hand, only the added finger has a distinct articulation with the metacarpal bone (fifth), which, in this case, is not split into two limbs. The articulation referred to is situated about one-fifth of an inch from distal extremity of the metacarpal bone.

D. *Right Hand.*—Here the accident is an abortive little finger, which hangs loosely, merely by a little tack of soft tissue, but without any joint. In addition, there is a webbed condition of ring and proper little finger, the union extending quite up to tips.

8. Annie (alive), Deformity of hands and feet.

A. *Hands.*—There are six fingers on each, the added fingers being abortive little ones, which are attached by mere tacks of skin.

B. *Feet.* — Here the deformity — and it is unique in so far as it is the only instance in the whole history—is in the great toes. Both feet have double great toes, *i.e.*, the elements of two great toes, so united as to form but one, on each foot.

In the *left foot* the individual phalanges can be felt to be quite separate. The nails also are separate.

In the *right foot* the contiguous individual phalanges are inseparably united at their lateral borders, thus forming one bone, which, however, is correspondingly broadened. Here there is only one nail which is notched in the middle of its free border, thus indicating primary division.

The great toes of both feet are excessively broad, and appear at a glance to be formed each of two united toes. Condition shown in fig. 6.

Generation II.—*Mr. M. and his brother and sister.*

1. M., Deformity of left hand and right foot.

a. *Left Hand.*—Has one finger additional, which arises by a distinct joint from distal end of fifth metacarpal bone. The metacarpal broadens out towards its distal end, as if about to bifurcate into two limbs, and thus supports the articulations for both the little and adventitious finger. For diagram of anatomical condition and sketch of hand, see figs. 7 and 8.

b. *Right Foot.*—A little toe additional. The fifth and adventitious toes, moreover, are united together by the adjacent soft tissues. The phalanges of the individual toes seem also to be united into single bones, but the nails remain separate.

2. Mr. M.'s brother (unmarried). Has deformity of both hands and both feet, as follows:—

Hands.—Six fingers on each—the additional digit in each being a small one.

Feet.—On one foot, six toes—on other, *seven!*

3. Mr. M.'s sister. Not deformed.

Generation III.—*Mr. M.'s father and uncle.*

1. His father, Deformity of left hand and right foot.

Hand.—Supernumerary digit resembled somewhat that on Fanny's left hand, but projecting as it did at a greater angle. Mr. M. says nearly a right angle—was a source of continual inconvenience to him.

Foot.—Had six complete and separate toes.

2. His uncle (married), Deformity of left hand and right foot.

Left Hand.—Had six fingers, and all the fingers of hand were united to tips.

Right Foot.—Six complete and separate toes.

2 a. His uncle's family. Consists of eight members—five daughters and three sons. All have the quadruple deformity—viz., six fingers on each hand, and six toes on each foot. One son had fingers webbed.

PLATE I

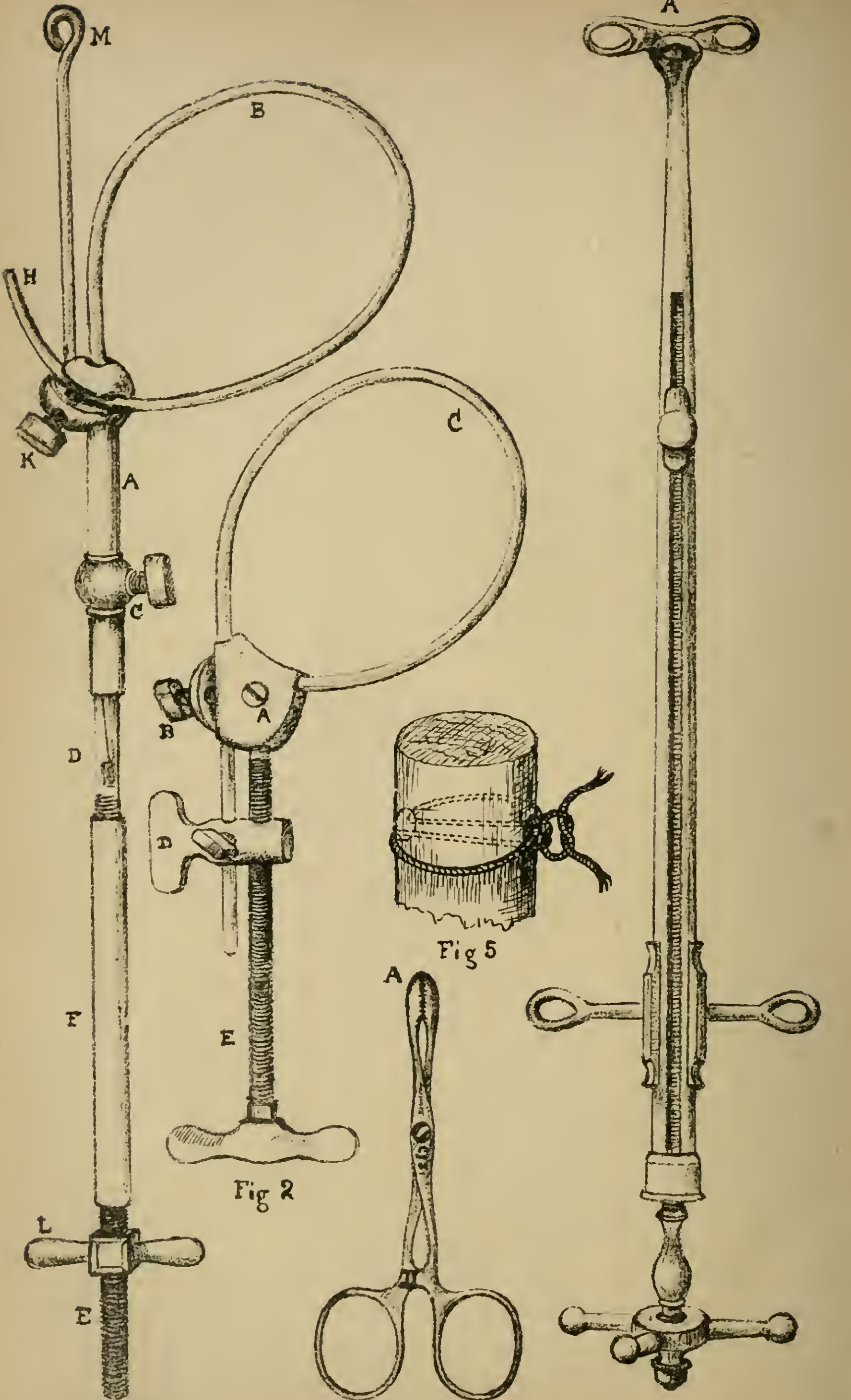

Fig 1 Fig 3 Fig 4

J. STUART NAIRNE'S *Wire Clamps, Modified Ecraseur, and Artery Forceps.*

Generation IV.—Grandfather.

Generation V.—Great Grandfather.

Both Grandfather and Great Grandfather, Mr. M. asserts, on the authority of his Father, possessed the deformity in some form or other, but he cannot tell the exact nature which it assumed in either.

ABDOMINAL SECTION AS PART OF THE SURGICAL TREATMENT OF SOME DISEASES OF THE ABDOMINAL ORGANS.

By J. STUART NAIRNE, F.F.P.S.G.

(*A Paper Read before the Glasgow Southern Medical Society*).

I.—Diagnosis of Intra-Abdominal Disease.

Diagnosis, at all times difficult, is especially so in diseases of the organs contained within the abdominal walls, or lying in the pelvis. Peritonitis, limited, local, general, are very convenient terms, and sufficiently instructive and distinctive to point to a general line of treatment; but they are neither so definite nor so accurate as to enable one to devise or carry out any line of treatment that might be specially applicable, and therefore more likely to be beneficial to each individual case. For example, there may be a collection of serous or other fluid in the Fallopian tube distending it, and causing inflammation of the investing peritoneum with the usual accompaniments of pyrexia and pain, constituting, in fact, vaguely a localised peritonitis: but see the difference between the terms localised peritonitis and hydro-salpinx, or pyo-salpinx as the case may be. The one is indefinite, indistinct, there is nothing pointed out except the presence of an inflammation somewhere: the other is definite, distinct, the very situation of the disease is indicated, and its whole natural history and treatment shadowed out.

Many people exclaim against the multiplication of names for diseases, as merely tending to obscure the general view of medicine, or, perhaps, having little or no anatomical or physiological foundation for their origin, have been invented to air the superior classical knowledge of some, or gratify a distorted whim for what at least has the semblance of being new.

But in spite of these things being granted to be true, a new name, which is a good name, playing the part of an accurate definition, either of a condition or operation, is a decided boon to the science of medicine. When, for example, Marion Sims for the first time opened the gall bladder, and called the procedure "cholecystotomy," he gave us a name at once accurate and comprehensive, and which is very unlikely ever to pass out of the annals of surgery. New names are a necessity of more accurate differential diagnosis, as well as of new operative procedures: and the best way to secure and advance one's knowledge, especially with new names, is to get to know what they exactly mean. If a new name does not mean anything exactly or definitely, it is not worth remembering; if it does, then you have the satisfaction either of making an addition to your knowledge, or of packing your previous knowledge into more convenient compass. As Andrew Fuller says, you can carry more knowledge if you have it "well fardled up," than you can in loose bundles.

To diagnose the presence of any abnormality in the abdomen or pelvis, one ought to be well posted up in the condition and position of the parts in health. Opportunity for practice in this respect lies at every one's hand in the cases of parturient women whom he attends. For more than a dozen years I have not omitted to examine every woman carefully, several times at least, at whose confinement I have been present: and external examination is that from which you may learn most. Vaginal examination is frequently unsatisfactory; and although I am far from saying that it ought to be omitted, it is frequently of less value than external examination. At any rate, for true intra-abdominal disease, speculum examination is utterly worthless. Two things surgeons must come gradually to employ less and less, the speculum and the sound. Touch is not inferior to sight; and the sight of the os uteri could never give you the information that you may have in one moment by the tip of the finger. The speculum is of use for surface affections of the vagina, for affections of the os uteri, and for the application of medicaments. There is no reasonable ground whatever for its employment, if the trouble is *above* the roof of the vagina. In fact, to insert a speculum into a patient suffering from prolapsed ovary, or Fallopian tube distended with pus, is a piece of unpardonable cruelty. As to the sound, its use should be restricted to ascertaining the position and depth of the uterus, and should terminate there; and should not be poked up invariably, unless there is reason to believe that there is some malposition of the uterus, or enlargement due to a cause other than pregnancy.

In fact, the only reliable method of diagnosis of intra-abdominal disease is that of manual examination, first altogether externally, and then with one or two fingers in the vagina and the other hand over the pelvis. In very many cases external palpation and percussion will determine the condition of the parts quite positively.

It is usually very easy after the birth of a child to examine fully the whole abdomen and pelvis. You can trace easily the broad ligament, the Fallopian tubes, and find the ovaries. You can press backwards and feel the iliac arteries, and press on the sigmoid flexure. You can trace round and round the uterus, and grasp it in your two hands: and you will find it not at all smooth, but nodulated and unequally contracted: and you will be able to determine the position of the *post-partum* uterine painful spot. With reference to this spot, to which great importance is to be attached, as I hope to be able to show farther on, I quote from the original notice I gave of it in the *Brit. Med. Journal*, 17th January, 1880—"After confinement, if the hand be placed on the abdomen, the uterus in general will be found contracted into a hard ball, longer than it is broad, and somewhat tapering from above downwards; and, if careful manipulation with pressure be practised, there will invariably be found one spot exquisitely tender, the remainder of the uterus, grasped and kneaded, remaining comparatively insensible. This spot may possibly not be found immediately after delivery; but I have never failed to find it within the twenty-four succeeding hours. This spot does not always occupy the same situation, but will be found sometimes on the left side, sometimes on the right side, sometimes a little more to the centre, and occasionally a little lower down, trenching on what, by external palpation, might be deemed the neck of the uterus. The commonest sites, however, are the right and left sides, at a distance nearly between the middle and upper thirds of the womb.

"This pain, which does not make its appearance till after the expulsion of the placenta from the womb, is persistent more or less for about eight days, becoming every day less and less acute, and at last totally disappears with the last remnants of bloody discharge, at least, I have not detected it afterwards. This painful spot is neither more nor less, in my opinion, than the position of attachment of the placenta."

Since writing this five years ago, my opportunities for observation and surgical practice enable me to corroborate more fully the truth of this theory; but I am not able now to say that this *post-partum* pain always disappears entirely. In some cases it does not disappear, but remains tender and sore, discharging sanious fluid occasionally, and is the evidence of a clearly defined intra-uterine disease, and amenable to its own special intra-uterine treatment.

When you have, after this manner, made yourself very familiar with the feeling of parts through the abdominal walls, you will find it comparatively easy, in most cases, to determine as to any pathological changes. In thin people, with lax abdominal walls, examination is easy, and the difficulty increases with obesity. In many thin people you require no vaginal examination at all to satisfy your mind; but in obese people you can rarely do without it, and may not be very sure even then what you should say.

If you think you find a tumour or lump of any kind in the abdomen, you must endeavour to get your hand below it. You will not be able to do this in many instances, but you will be able to do it so far as frequently to satisfy your mind that what you have taken for a tumour is nothing but a bundle of adipose tissue in the walls of the abdomen.* The patient must take long breaths, and, during the prolonged *expiration*, your hand must press down the abdominal wall at a little distance from the supposed tumour to allow of your getting behind it. This manœuvre requires care; but, with anything like moderate care, no possible harm can be done. It would be impossible, for example, except with great carelessness, to mistake an abdominal aneurism for another tumour, and to press on it so violently as to cause rupture.

It is easier to find an enlarged or tender ovary than a normal one. In fact, it is usually comparatively easy, with a finger on the vagina at one side of the uterus and the other hand on the abdomen, to get an ovary between the hands, and to determine its condition. To determine, however, the condition and connection of ovarian cysts is not so easy. It is impossible, frequently, by any ordinary methods of examination, to say anything definite about them. Liston, part iii, p. 52, expressed perfectly the same thing long ago—"The situation and attachment of such tumours cannot be correctly ascertained during life, far less can their internal structure and disposition be arrived at; indeed, an accurate diagnosis is exceedingly difficult, if not impossible. Innumerable mistakes have been made, which have led to most unjustifiable proceedings. In

* Scarpa *On Hernia*—Wishart's Translation, 1814, p. 392, *et sq.* pp.

one case the abdomen was, after two or three dry tappings, opened by an incision from the ensiform cartilage to the pubes; the viscera were turned over and over, but no tumour could be discovered. The woman was sewed up and did not die."

There could have been no possible excuse for such a proceeding as is here indicated. An incision of two inches would have settled the question as to the presence of a tumour, as well as one of twenty inches. Errors, nevertheless, will always occur in diagnosis; some, from the intrinsic difficulty or obscurity of the case—excusable error this; and some from sheer ignorance, carelessness, or general stupidity—errors inexcusable. Facility and accuracy in diagnosis can only be gained by long experience; and long experience counts for nothing unless conjoined with care and attention. I operated on one case which had been diagnosed as a suppurating cyst, and the operation disclosed a serous cyst. Another case, diagnosed as ascites, turned out to be an internal hæmorrhage, due to ruptured omental vessels. I operated on a case diagnosed by four medical men as retroverted uterus, and found a prolapsed ovary. This last is a common mistake. I saw, a short time ago, a case exhibited as a case of retroversion of the uterus, where the sound passed into the uterus easily towards the front to the normal depth without removing the posterior painful lump, which it would have done had it been a retroflexed or retroverted uterus. Such a case could be nothing but a prolapsed ovary. I operated on a case for pyo-hystero-salpinx, and found a hydro-hystero-salpinx. The respected President of this Society, Dr. Park, knows of a case aspirated for pyo-hystero-salpinx, which was a hæmatocele. Another case operated on as a supposed pyo-salpinx turned out to be a dermoid tumour of the ovary. I know of a case operated on as a fibroid of the uterus, which was a hydatid tumour. *Per errores lux;* Liston's time and this are not to be compared altogether. Since his time we have differentiated many things, and there can be no doubt that we are fast formulating the pathognomonic signs of clearly differentiated diseases, and that errors with regard to diagnosis will be proportionately diminished.

II.—Enumeration of Cases Suitable for Abdominal Section.

The opinion has frequently been expressed that exploratory incision of the abdomen (in preference to puncture or tapping) has been, and is being carried too far: and partly in anger, and partly in ignorance, abdominal surgery has been called "abominable" surgery. I consent at once that some abdominal surgery is well worthy of being called in question. When a man undertakes work for which he has not fitted himself by special and anxious care, when he has not made himself acquainted with the special tools required, and when he has not made himself at home in their manipulation, you could not look for any other thing than mere botch-work, and no journeyman work at all, and, alas, the results of such botch-work are misery and despair. A great man says the same thing, "If I chose to tell what I know of this operation; of the mistakes of diagnosis when there was no difficulty; of unfinished operations that could easily have been completed; of wounded bladders; of deaths from hæmorrhage, arising from the want of proper instruments, or from not knowing how to use them; and of, after all this, ignorance and self-conceit remaining perfectly satisfied—I am sure you would agree with me in thinking that so far as hysterectomy has thus gone, it would have been better that this operation had never been." * Amen and Amen. Let every man weigh his work well, and measure himself with his work, and see how he stands with it honestly and laboriously; and not flippantly, nor ignorantly, answer the question if the thing he proposes is the downright best thing for the poor sufferer who consults him. And the more carefully cases are studied and prepared for, I am sure we shall find that we shall be more frequently able to give succour when cursory examination and deficient knowledge would deny it.

Abdominal incision is anything but a new proceeding. I quote from Cooper's *Surgical Dictionary*, Art. "Paracenteses" (1813). "Notwithstanding authors generally recommend a small trocar for the purpose, there can be no doubt that dividing the skin and making a cautious puncture with a lance through the linea alba and peritoneum would be a preferable mode of proceeding. Incision is beyond all question a clear, and open, and definite way of seeing and knowing what you are doing, and ought, without doubt, to be preferred in all suitable cases."

It will be easy for me, I am sure, to establish the propriety of abdominal section at the very least, as an exploratory incision to determine the feasibility of an operation for the radical cure of a disease in the following cases:—

* *Brit. Med. Journ.*, p. 56. Vol. I, 1883.

Obstruction of the bowels.
Strangulated hernia.
Scirrhus at the pyloric orifice of the duodenem.
Scirrhus of the rectum.
Pelvic abscess.
Hæmatocele.
Purulent peritonitis.
Inexplicable pelvic symptoms.
Tumour of liver.
Distended gall bladder.
Tumour of spleen.
Tumour of pancreas.
Tumour of omentum.
Tumour of kidney.
Distended ureter.
Tumour of ovary.
Tumour of parovarium.
Tumour of broad ligament.
Distended Fallopian tubes.
Tumour of the uterus.
Pregnancy with distorted or narrow pelvis.
Placenta praevia—special cases.

These subjects are not arranged with any view to relative importance. They are all important; they represent diseases painful and fatal, and in some cases remediable. Operations for some of them have been done so frequently and successfully that they may only need mentioning; such are those for various tumours, ovariotomy being now an operation out of the region of discussion. Less frequently performed, and still to be more fully discussed, are operations for fibroid tumour of the uterus. Then there are operations for pyo- and hydro-salpinx—distension of the Fallopian tubes—urgently advocated by Mr. Lawson Tait. Then operations still less frequently done are those for tumour of the liver, and those for distended gall bladder (cholecystotomy — J. Marion Sims). Porro's operation has very well established itself for pregnancy with distorted or narrow pelvis. Strangulated hernia has found in Edinburgh treatment by abdominal section, proposed first by Mr. Tait. Hæmatocele and pelvic abscesses have been opened and drained. Obstruction of the bowels has been treated with no very great success by abdominal section, which is due, I have no hesitation in saying, to faulty performance.

Operations that have not yet been done, at least not satisfactorily, are those for scirrhus of the rectum. Resection of the pylorus has been done several times without an *immediately* fatal result—result, however, following after as soon as possible—an operation, I venture to say, both unscientific and useless, and strangely ignoring the only sensible plan of treating surgically malignant stricture of that part. Operation for exceptional cases of placenta prævia was proposed in this Society during the present session, and met with universal disapproval. I think this disapproval unmerited, and I still urge that there are special circumstances and cases of this nature that demand immediate operation. Of all these, however, in detail farther on.

III.—Statistics—their Value and Place.

To test the value of operative procedures, it is usual to appeal to statistics. New operations have usually been devised by hospital surgeons, and carried out first in hospital practice; but to this there are some notable exceptions, and chiefly in connection with abdominal and gynecological surgery. So far as known at present, the first attempt at ovariotomy was successfully performed by Houston, a member of the Faculty of Physicians and Surgeons, Glasgow, in private practice; and the immediately succeeding ones were done by M'Dowell in the back woods of Kentucky, far removed from the sound of any public hospital. Sir Spencer Wells' first case was done in the Samaritan Hospital in 1858. Dr. Keith's cases were done mostly in private practice. Marion Sims originated his treatment of ruptured perineum amongst the miserable slaves on the plantations, and since then the treatment of ruptured perineum has become a hospital operation, with no special benefit to those operated on. It is supposed to be well-known that the mortality statistics of hospital work are far above those of surgical work in private practice; but as a matter of fact, I believe this is only partly the case, and that surgeons generally, without paying much attention to the subject, take it in a half-hearted kind of manner as this, ever since the celebrated paper of Sir James Y. Simpson on the same subject. This is certainly not as it should be. If the mortality statistics of large hospitals are really unduly great, it is the bounden duty of every man in the medical profession to set his face against the erection and continuance of large hospitals. But, if the mortality be really not due to the *largeness* of the hospital, but to some other cause or causes, it is further his duty to help to elucidate these causes, and help on a reformation. If the mortality *must* really be exceptionally great in a hospital, then the poor miserable wretched people who go in there ought to know it; and if the rich will not or cannot help them, they should help themselves.

Some time ago, I had the honour of bringing before this Society a record of three hundred miscellaneous surgical cases occurring in my private practice, and treated entirely by myself amongst my ordinary work. Part of the concluding statistical division of that paper I here repeat, partly for the purpose of emphasising the necessity there is for more home surgery, but chiefly as indicating in some measure the differences that exist between private and hospital practice, and pointing out very clearly that large percentage mortalities are not due altogether to the simple *greatness* of a *building per se.*

In order to have a clear view of this division of our subject, I tabulate regarding hospital mortality (surgical cases) the following propositions, so that at least there may be no doubt about what I mean.

1. Hospital mortality is calculated on *too short a term* to be of the slightest value. The average residence of patients in our infirmaries here is from 29 to 31 days.

2. The adding together of trivial and severe cases for totals, to calculate death percentages from, is utterly absurd.

3. The published average mortality (usually about 3·5 per cent), is totally misleading; in fact, untrue.

4. There is, as matter of fact, really no percentage mortality in trivial cases.

Of course, deaths occur occasionally in trivial cases, but certainly not in such number as to entitle them to be percentaged; otherwise, indeed, such cases would soon cease to be called trivial.

5. The percentage mortality of serious operations in hospital practice is seldom less than 60 per cent.

In my 300 cases, amongst which there were 97 serious operations, the rest being trivial, I had only 3 deaths during the first month after the operations, one on the table, one on the third day from tetanus after removal of a small uterine polypus by torsion (a very bad method of removal, I must say), and one a week after operation. During the year, or two years succeeding an operation, death has not unfrequently occurred, sometimes from the same disease, sometimes from exhaustion, and sometimes from another disease altogether. In fact, to put this statement very plainly, death frequently follows operations for disease in the course of one or two, or three years. If an operation has been necessitated by an injury, and if the sufferer recover, there is every reasonable prospect of the recovery being permanent, and death only resulting in the natural course of time. But, if an operation has been required for disease, for strumous disease of the bones, for malignant disease of the glands or other organs, then the operation will rarely result in a perfect cure; but the disease will sooner or later break out somewhere, and hasten the termination of the patient's existence. Operations for disease stave off death for a limited time, rid the patient of pain and suffering, make life more bearable, make life sometimes even valuable, restoring the invalid in some part to his place in society, and enabling him to perform his duties. But while he is thus cheered and strengthened by his immediately resulting condition after recovery from an operation, the hope proves but too often delusive, and we have the inexpressible grief of finding such cases drift uncontrollably beyond all surgical and other aid, and end their days in misery and despair. How often have I seen cases of cancer of the breast dismissed "cured" from hospital, in a few short months the most desperate objects of commiseration, lying waiting for death. Dismissed cured! Could there be any greater absurdity? Neither private nor hospital practice can CURE; but private practice gives fewer deaths immediately after operation.

Hospital work has its place. It is a great boon, and a great help to the surgeon who can use it; and it ought to be a great boon to the poor people who require to take it. How are both to be best benefited?

J. Marion Sim's *Uterine Surgery*, p. 331, says—"When asked if it was possible to cure it, I said, 'I do not know, for the books throw no light on the subject; but it appears to me that the only rational treatment would be surgical. However, I declined to do anything, on the ground that an untried process was not justifiable on one in her position in society, the hospital being the legitimate field for experimental observation." *The legitimate field for experimental observation!* Such a statement requires a large amount of qualification. Is a poor factory girl, by no fault of her own born in the lower ranks of life, and who has nothing but her two hands to earn her bread with, a more *legitimate object of experimental observation* than the lady, born by no credit of her own in the higher ranks of life, and who needs not to soil her fingers to obtain every luxury the human heart could wish? Coming thus to particulars, I trow not. The one is as little to be experimented on as the other, and rich and poor alike are sacred. Rich and poor are alike at liberty to choose, if they please, the risks of an untried or doubtful operation; and certainly, above all things, no one has a right to do in hospital practice any operation that he would not advise or do in private practice. Human life is the same all over. There is not one life of the poor and another life of the rich.

I have no desire to go over the same ground in statistics that I formerly did; but as I know that there is still a very false impression generally as to the results of serious operations in hospital work, I will repeat a part from my examination of one hospital report. *Ex uno disce omnes.*

This is the report for the Glasgow Royal Infirmary, 1882.

Removal of abdominal tumours 2; died 2.

Removal of foreign bodies 7; cured 7.

The per centage mortality in the first case is 100; in the second case 0; the average per centage is 9 operations and 2 deaths = 22·2 per cent. When, however, you examine the cases of removal of foreign bodies, and find fish bone in throat, needle in finger, you cannot help smiling at the innocence that marks death by such recoveries. To go a little farther however, the mortality of "major operations" is put down at 15·29 per cent, into which calculations the following operations do not enter.*

Excision of mamma,	11 operations;	2 deaths	= 18·1 p.c.
Excision of abdominal tumours,	2 operations;	2 deaths	= 100 p.c.
Spina bifida,	2 operations;	1 death	= 50 p.c.
Gastrotomy,	2 operations;	2 deaths	= 100 p.c.
Herniotomy,	9 operations;	6 deaths	= 66 p.c.
Ovariotomy,	5 operations;	2 deaths	= 40 p.c.

These give an average per centage mortality of 62·4; and now add in the published mortality of major operations 15·29, and there is a general average mortality of 55·7 per cent in serious cases.†

Such per centages are quite shamefully too large, and point clearly to several things—viz., that the bulk of these operations have been done in extreme cases, where the patient must have been already almost *in articulo mortis*, or in unsuitable cases, or inefficiently done, or improperly attended to after they were done, or done under insanitary conditions which militated against a case; and not one of these several things is justifiable, with the single exception of the first, which is improbable.

Hospital work is done in large, airy buildings, with the

* Nothing could show more conclusively than this that it is high time "major" should cease to exist as a surgical term.

† In case I might have lighted by accident on a specially bad year, I have glanced over the last three years' reports and find the results in the same classes of cases worse, if anything, than the above.

possibility of all appliances and assistance, and ought therefore to be more efficiently done than surgical work in private practice. I have myself been driven to every kind of expedient to save time and assistance. It is not possible, unless you have a hospital appointment, to have the same facilities for your work as hospital surgeons, and hospital work ought therefore to be better than private work, but it is not. My work has been done in the homes of the people generally, with such assistance as I could most readily get. General practitioners have an erratic and uncertain time of it, and I have frequently been left to tackle with a piece of work myself by the non-appearance of some professional brother, urgently or otherwise detained. In this one great and important respect these statistics are of value as determining in a general way all over, that your surgery done at home, even under adverse circumstances, will at the very least be sure always to equal hospital work. You ought not then to be deterred by misleading hospital reports from doing the work as well as you can at home, and you will have your reward. I agree partly with Sims, the hospital is a field for gaining practice and experience, certainly not for performing experiments.

From a mixture of cases, then, considered for a limited period, you can draw no valuable conclusions, in fact, no reasonable conclusions at all. Everything depends on the kind of case. If you could get fifty or a dozen cases precisely similar, you might infer something general concerning these; but you cannot get even two cases that correspond sufficiently with each other to enable you to do so. This is very specially the case with abdominal surgery, and statistics with regard to so-called "same operations" require to be very carefully considered in the light of each individual case, considerations frequently resulting in very important corrections.

If a man have sufficient work to allow him to pick his cases, he will frequently choose those that seem likely to do well, and leave the desperate cases either in the hands of providence or to find their way to some more needy or daring operator. The statistics of the first man may, therefore, appear better than the other's, but he has not made the same effort to relieve suffering as the man with the fewer recoveries. Many of the operations that I have done were done in extremity; and, if death followed, as it does not necessarily do, it was not so much a surprise as the recovery was a gratification. I lay this down as a law independent of statistics, that it is the duty of every man, in desperate cases, when ordinary means of cure have failed, to take up extraordinary. In a case of pelvic abscess, for example, it is the only right thing to do to open and drain whether the patient recover or not. You do the best for your patient. Of course, you may stand fussily aside, uselessly busy, and see this great sight, as I have too frequently seen, of cases "left to nature" to suffer from hectic, from sweating, from pain, till the abscess burst somewhere, perhaps into the bladder, the bowels, or through the abdominal walls, or into the peritoneum, carrying with it speedy death or more prolonged suffering. I do not call this surgery. Art is here not the handmaid of Nature, which is making for dissolution through suffering, but a spectator interested or otherwise of the agonies of life. Do not leave obscure abdominal cases so much to Nature. Depend on it, Nature does not know much better than you what to do.

Do not be deterred by "statistics" from doing what you conceive to be your duty—statistics is a kind of "bogle" set up by the disingenuous, mostly as a signal mark of their own ability, and a dreadful "scare crow" to others. Distrust them; they may mean anything, they may mean a selection of picked cases, of easy cases, of cases that would have got better under almost any one's hands, of young, healthy, vigorous patients; they may be an indication certainly of dexterity on the part of the operator, gained by experience, or they may be a freak, a run of successful cases with no special merit to the operator. It is, indeed, high time that this statistical curse were abolished, if it cannot be put on a proper basis, and the miserable quibblings about one man's work, in comparison with another's, brought to a final termination. The justification of any operation is its necessity, and not the numerical recoveries from its performance: but we are all inclined to shirk operation in desperate and unlikely cases when we can, in case we are blamed for hurrying on a fatal termination, or for fear that the patient die under our hands, or immediately after. These things would not move me, and I am able to say from experience, that in such cases, by honest, straightforward outspeaking, a man can always save himself from the reflection of friends. So far as I am concerned myself, I know of only one case where I was blamed for operating. It had been a case of excision of the breast for scirrhus, and the glands in the axilla had become subsequently affected. She had made a very rapid recovery from the former operation, and insisted on having this one done. Perhaps I had not explained fully enough to her and her husband the additional danger of such a case; but, at any rate, I had not neglected to warn them altogether. Dr.

Napier kindly assisted me on this occasion. At the operation the glands were found extensively infiltrated, extending deeply below the clavicle. In endeavouring to detach these glands, the subclavian vein was ruptured, and the tissues were so friable that they would not hold a ligature. She died within an hour. This was some years ago, and I had not then become sufficiently familiar with pressure forceps to make proper use of them, else, I daresay, this poor woman's life might have been saved.

However, just as some people will blame you for putting on a fly blister or for not putting one on, so you may expect that you will be blamed sometimes for operating and sometimes for not operating. The course to be adopted, nevertheless, to have the least possible blame, is quite clear, and that is, in every case, to make the patients and their friends aware of the risks, and to *cause them to acknowledge* the risks before accepting them; and then, if the operation be accepted, never mind statistics; do the best you can if you are determined to practise surgery. But there could be nothing more cruel or heartless of any one than to undertake a severe and serious operation, who has not his mind made fully up to go on and perfect himself in the work. A serious operation will call up all your reserve forces; and to an infinite degree more so as a private practitioner than as an hospital surgeon, where your responsibility is so much minimised. This feeling of responsibility is frequently overwhelming, and has the tendency to take the pith out of a man. Ignorance and loss of self-possession are the chief causes of distress. A man cannot look to get well through an operation who does not know what he may expect to find; or who has not studied his part diligently. Nothing could illustrate better what I mean than a story told me by brave old Dr. Borland, of Kilmarnock. "In the early days of my career (the jubilee of Dr. Borland's career was celebrated in Kilmarnock several years ago), I was asked by a neighbour to go into the country to assist him in an operation for strangulated hernia. There were three of us. The operator was called 'Jock.' I knew very little about strangulated hernia, and my friends knew as much. The operation went on very well; by and bye the operator opened the sac, and out gushed some reddish serum. Everybody at once thought the bowels had been cut. 'God! Jock, whispered my neighbour, ye've done for him noo—shoo him up,' which advice was incontinently followed, and we got out of the house as quickly as possible, after telling the people the man was sure to die."

Here, then, was ignorance and timidity, a bad conjunction certainly, but I think hardly so bad as ignorance and rashness. In a similar case (strangulated hernia), I knew one operator who, at one stroke of his knife, went through skin, subcutaneous tissue, sac, and bowel. The bowel was stitched and returned, and the man died.

A distinguished surgeon says, "I have now finished my fourth successful case of ovariotomy. I attribute my success to greater care in the selection of my cases."

Now, while there is a certain element of value underlying this statement, it is one that has frequently no right to be made. One would like a statement something like this—"I have now finished my fourth, or fifth, or sixth, or such as it may be, unsuccessful case of ovariotomy. I attribute the failures to the causes hereinafter mentioned." As I have already said, the cases that are picked as likely to do well under operation are invariably quite as likely to do well for many a long day without any operation at all. So that here, in order practically to tabulate successful results, a man deliberately sends away the poor, and wretched, and miserable, whose lives are a burden to them, and does not offer them the resources of his art to the utmost. Above all things in the world, a man with a remedy has no right to pick or choose in this manner; he has no right to follow the example of a great French lithotomist who was publicly blamed for sending away all the bad cases lest they should invalidate his statistics.

This is, indeed, one great fallacy of statistics, that recoveries from an operation do not always guage the value of an operation. The more shame to a man who will not undertake desperate cases for such a craven fear as this. Why, he may deserve more credit for his unsuccessful cases than another for his successful ones. One such desperate case, snatched as it were from the jaws of death, is more glorious than ten whose lives were never in jeopardy. When any one then asks, "How many of such and such a kind recovered?" he ought to be reminded how many were dying, how many were suffering in such a degree as to prefer death to life, how many, although not dying, had death immediately before them, and chose to meet him half way under the chance of escaping for a longer while.

I repeat again, the justification of an operation is not the numerical success in recoveries of picked cases; nor is the condemnation of an operation to be found in the numerical failures of the cases taken as they come; but the justification of an operation I venture to put down under the following dicta:—

1. That the operation be practicable, *i. e.*, that it be possible to do it without the certainty of killing the patient.

2. That death is approaching, or that intolerable suffering is making life unbearable.

3. That the patient knows and takes the risk.

4. That there is a prospect of cure or relief.

Under these circumstances throw statistics to the dogs and do the best you can, without fear and without rashness. It is too much the habit to be proud of the cases that recover after operation, and it is distinctly too much the custom to report only these. In doing our work we ought to know that the issues of life and death are in His hands, and that we acquire no discredit if we have done our work as well as we can should our patient not recover. After almost every operation a thoughtful person is inclined to say, "if I had done this, or that, or the other thing instead of what I have done, it might have been better," and still, to our astonishment, we find that such doubtful cases frequently recover, and others that we have been better pleased with die. But there are few operations which, if we had to do them over again under similar conditions, we would not be inclined to modify in some respect. Doubtless we frequently have ourselves to blame somehow when a case does not get on well, but I emphatically protest against the disingenuousness of concealing these and only proclaiming the successful ones. I have in my mind at the present moment an operator who did three abdominal sections, two died and one recovered, and the one that recovered was the only case of the three that appeared in print.* I saw another surgeon operate on two similar cases of tumour requiring section of the lower maxilla; one died, the other got better; the one that recovered was reported, the report of the case that died was evidently interred with the remains of the patient, for it never appeared.

EXPLANATION OF PLATE 1.

Fig. 1. Clamp. AF, Tube through which screw E works by handle L.
F, Part of tube removable along with screw E.
B, Nickelled wire, passing through tube A, dovetailed into end of screw E.
H, End of wire drawn tight and passed into collar, and pinched with pinching screw K.
M and A, Bearers for extra-peritoneal treatment of pedicle.
C, Pinching screw for wire B when drawn tight.

* I have had my attention directed to the fact that this operation has appeared in a tabulation as 1 operation and 1 recovery, whereas I know it was 3 operations and 1 recovery, a very different statement of affairs indeed. *Sic pudor fugit.*

Fig. 2. Clamp. c, Nickelled wire, pinched at A and brought round through collar at B.

E, Screw.

D, Sleeve nut pinched on wire. After the wire is drawn tight, screw and sleeve nut are removed, and the wire C with collar A alone remains.

Fig. 3. Kœberle's forceps, modified at point A to permit of the easy application of a ligature.

Fig. 4. Ecraseur, with modified head, A, for passing a thick cord round the base of a tumour to secure compression without the risk of cutting the tissues.

Fig. 5. The Staffordshire knot.

I have to thank very particularly Mr. Edwin Gossman for the great trouble he has taken in elaborating and making these instruments for me in a beautiful workmanlike manner.

CURRENT TOPICS.

Foulis Memorial Scholarship.

This Scholarship was founded in 1882, in memory of the late David Foulis, M.D., Glasgow, who had distinguished himself in Pathology and Surgery, and died at the age of 35 years. It is to be awarded as often as the accumulated income from the invested funds amounts to £50. It is tenable by a graduate or licentiate whose professional education has, to the extent of not less than two years, been received at the University, or one of the extra-mural schools, of Glasgow, and who shall, in the opinion of the trustees, have manifested a capacity for original work in the department of pathology. The purpose of the scholarship is to assist in the prosecution of such work in one of the medical schools furth of Scotland, approved of by the trustees; and the trustees have power to determine whether it should be awarded by competition or otherwise.

The Trustees are the Professor of Practice of Medicine in the University, the President and Visitor of the Faculty of Physicians and Surgeons of Glasgow, and the Pathologists of the Western and Royal Infirmaries.

The first appointment will be made early in October 1884 and names of candidates are to be sent in to Mr. Alexander Foulis, 180 St. Vincent Street, not later than 1st September. The application will state the time which the candidate proposes to spend, the place of work, and the probable character of the proposed investigations.

At a Court of Assistants of the Worshipful Company of Grocers held on Wednesday, 21st May, 1884, the following candidates were elected to the Company's scholarships for research into the causes of important diseases, viz.—

1. W. North (renewal). *Subject*—Ætiology of Ague.
2. Leonard C. Wooldridge, D.Sc., M.B. *Subject*—Hæmorrhagic Disease, Pernicious Anæmia, and Blood-clotting.
3. Alfred Lingard, M.R.C.S. *Subject*—Intimate Ætiology of Enteric Fever.

REVIEWS.

Fünf Lustren Ophthalmologischer Wirksamkeit. Von Dr. Albert Mooren, Düsseldorf, Wiesbaden. Verlag von J. F. Bergmann. 1882.

Twenty-five years of Ophthalmic Practice. By Dr. Mooren, Düsseldorf.

Dr. Mooren is favourably known as the author of numerous important papers on ophthalmological subjects: a volume of clinical observations — *Beobachtungen* — published in 1867, giving the results of the first ten years of his professional work, and a monograph on sympathetic ophthalmia, which was reviewed in this journal. The present volume gives an account of twenty-five years of eye-practice, including those of the *Beobachtungen*, *i.e.*, from 1856 to 1881. As in the *Beobachtungen*, there is a classified index of the different diseases of the eye, with their relative numbers in each year, and the proportion of cases in which one or both eyes were affected, with extended remarks on the more important diseases, intended to present, in a somewhat fragmentary form, a short exposition of those views which have formed the basis of the author's medical and surgical treatment. Twenty-five years of professional work, with the exceptionally large number of 200,000 cases in a single speciality, afford ample time and abundance of material for the intelligent observer to acquire a competent knowledge of the clinical aspects of its diseases, as well as to test the value of the various therapeutic agents at his disposal. A faithful record of the experience thus acquired, and the expression of the matured opinions of a painstaking and judicious practitioner on the general principles of diagnosis and treatment, give to productions of this kind an exceptional and permanent value. Moreover, if guided by a sound pathology, and gifted with an insight into the nature of those morbid processes which underlie diseased action, and give to its symptoms their distinctive character, we should expect—if, indeed, the very abundance of the material did not present serious obstacles to the carrying out of that minute and careful scrutiny so necessary to the discovery of truth—some contribution to the advancement of general pathology and treatment, and to the final settlement of those disputed theories which still obtain in nearly all the diseases of the eye. But Mooren, dealing as he does mainly with the clinical aspect of disease, is for the most part content to accept the results obtained by the experimental and pathological investigations of others, and to correlate them with his clinical experience. But the complicated relations existing between the objective symptoms of a given disease of the eye, and its associated pathological conditions, render it at all times exceedingly difficult to determine exactly those changes which are secondary and those which are primary and essential; for that can only be done satisfactorily when the symptoms and pathological condition can be shown to be intimately connected, and to be invariably present under like conditions. The neglect of this important element, and the too great reliance upon the observations of others, occasionally, as we shall see, lead the author into certain inconsistencies which might otherwise have been avoided. So much, then, by way of preliminary observations; we shall now proceed to notice some of the more important diseases. Under the head of diseases of the eyelids and orbit, Morbus Basedowii, on account of its interest both to the general practitioner and to the specialist, naturally occupies a foremost place, and is treated of in some detail. Adopting Sattler's view, that the disease can be accounted for in the first instance by a functional disorder of the vasomotor centres, he thus explains the occurrence of the three prominent signs—palpitation, goître, and exophthalmos; but as one or more of these signs may be absent, none of them can be regarded as pathognomonic, therefore, Mooren insists upon the importance, from a diagnostic point of view, of the upper eyelid remaining stationary in the downward glance, thereby exposing a portion of the sclerotic at the upper margin of the cornea. This sign, which *per se* is of no clinical significance, yet when associated with any one of the three well recognised signs, he regards as being almost pathognomonic of this disease, and states that he has found it existing in both eyes, when the exophthalmos was present only on one side. The author does not suggest much which is new either in the ætiology or treatment, and states that durable results can only be obtained when treatment can be directed against well-known causes, *e. g.*, anæmia, &c.

Diseases of the Lachrymal apparatus.—In watery eye, occurring with complete patency of the tear passages, Mooren still adheres to his original opinion that the morbid lachrymation is due to hypersecretion from the conjunctiva, and when it occurs late in life, he regards it as a symptom connected with glaucoma—both conditions being due to irritation of the ciliary nerves. In catarrh of the sac and nasal duct, fomentations and instillations of weak carbolic acid diminish the swelling of the mucous membrane and the secretion of mucus. He follows up that line of treatment by injections of weak antiseptic solutions, and only on rare occasions employs Bowman's probes, as he does not consider that the mere fact of the existence of obstruction, apart from irritation of the mucous membrane, will account for the stillicidium.

In cases of obstinate blennorrhœa and fistula, obliteration of the sac gave the greatest and most permanent relief to the patient, and after having tried the various methods recommended, Mooren gives decided preference to the galvanocautery, as being the most easily applied, and producing less inflammatory reaction than is apt to occur with the other methods.

Cornea.—After some very judicious remarks in the treatment of injuries and ulcers of the cornea, the author proceeds to the consideration of keratitis profunda, which he regards as almost certain evidence of syphilitic taint. He cannot, however, look upon the notching of the teeth as pathognomonic, but as due to the manifestation of the syphilitic virus in the system during the development of the teeth, and in some cases he has traced the syphilitic infection back to the fourth generation. In the treatment he prefers mercurial inunctions, and, as it is a chronic disease, he contents himself by employing fomentations in preference to any energetic local treatment, and only makes use of stimulating remedies when the disease has reached its later stages.

Iritis.—Mooren's statistics show that out of 2,068 cases of iritis, only 169 could undoubtedly be traced to a specific virus —a much smaller proportion than syphilis is usually credited with. He justly condemns the indiscriminate use of atropine, and bases its employment upon the presence or absence of congestive symptoms. When the iris is congested, he believes that atropine aggravates the disease, because by its paralysing influence on the blood-vessels, it tends to increase exudation, and especially is that so when the iris is already infiltrated, as it is apt to be in the syphilitic form. The administration of alteratives and derivatives, with fomentations locally, generally suffice to combat the active symptoms, after which he finds that, as a rule, the adhesions readily break up under the use of atropine. In those cases where, in spite of that treatment, the adhesions still remain, he, like most judicious surgeons of the present time, prefers to wait until the inflammatory symptoms have subsided before he performs an iridectomy, and regards the occurrence of inflammatory exudation threatening to close up the pupil on the third or fourth day, after a carefully performed operation, as a certain indication of a syphilitic iritis.

Sympathetic Inflammation.—Every acute inflammation localised in any part of the uveal tract, and culminating, as it is apt to do, in a cyclitis, is the essential condition for the propagation of a sympathetic disturbance in the other and hitherto sound eye. That, with certain reservations, was the doctrine formerly enunciated by Mooren. These reservations apply to the optic nerve, which, in some cases seemed to be the medium of transport of the sympathy. This view, founded on clinical observation, has in his opinion been amply confirmed by the pathological researches of Alt, who found swelling and prominence of the optic nerve in 79 per cent of the eyes enucleated. Accepting these observations of Alt, and adopting the theory of Cohn, that inflammatory irritation in any of the branches of a ciliary nerve will suffice to excite a sympathetic disturbance, he finds ample confirmation of his views derived from clinical observation—viz., that a sympathetic ophthalmia may be generated through the ciliary nerves, the optic nerve, or both together. If, as he admits, injury or disease of the uveal tract is by far the most frequent cause of sympathetic inflammation, the participation of the optic nerve must be accounted for by the intimate anatomical and nutritive relations existing between the choroid, optic nerve, and retina, and, therefore, when present, it is not necessarily an essential factor in the propagation of the sympathy. In the cases cited, where the sympathetic inflammation followed enucleation, the pain felt in the stump ought rather to be referred to the ciliary nerves than to the optic nerve, which does not, as a rule, by irritation, give rise to sensations of pain. Indeed, the author seems to have overlooked the fact that the ciliary nerves are also vasomotor. As regards the all important question, why sympathetic inflammation follows in one case, and in another precisely similar case there are no symptoms of sympathetic disturbance, the author can only suggest that the vulnerability of the sympathising eye in the one case is greater than in the other, and, therefore, we cannot say, in a given case of injury, that it will or will not excite a sympathetic inflammation, and seeing that enucleation, to be perfectly successful, must be performed early, and at a time when it is impossible to distinguish between a simple irritation and a genuine inflammation, many eyes must necessarily be sacrificed, which might possibly have been retained, without danger to the other eye. When the vision of the injured eye is irretrievably lost, that is of less consequence; but it is otherwise when there is a prospect of saving the injured eye, even when the iris and ciliary body are seriously implicated—a not by any means uncommon case. Since having adopted and put in practice the theory of Cohn, he has not had to record any loss from sympathetic inflammation, and, considering the wide distribution of the orbital trigeminus, he is satisfied that nothing short of enucleation can afford a sufficient guarantee against the possibility of a sympathetic inflammation.

Glaucoma.—The increase of tension, as a consequence of disturbance of the balance of secretion and excretion of the humours of the eye, is now regarded as the only reliable symptom of glaucoma. Before the discovery of the lymphatic channels in the corneo-scleral region, and the determination of the nutrient currents within the eye to this point, the increased tension was referred to a pathological increase of secretion either by inflammatory process or secretion neurosis. The discovery of the excretory channels in the ciliary region, and of their more or less complete obstruction in glaucoma with the consequent retention of the secretions—possibly normal—gave to the chief symptom a purely mechanical character, and at the same time afforded an explanation of the therapeutic action of iridectomy and sclerotomy, which, by the partial opening up of the passages, restored the equilibrium of the fluids. After referring to the pathological observations of Knies, who found in 15 cases a circular adhesion of the iris periphery to Descemet's membrane, obliteration of the canal of Fontana and a cellular infiltration in the neighbourhood; to the experiments of Weber, who, by injecting oil into the anterior chamber, artificially closed Fontana's space, increased the tension of the eyeball; and, on the other hand, to those of Schöller, in which a glaucoma was developed in the rabbit by cauterising the corneo-scleral region, and which he has observed as the results of severe burns in the human subject, Mooren goes on to say that pathological anatomy, experiment, and clinical observation, all combine to put the fact beyond a doubt that a more or less complete impermeability of the filtration channels in the corneo-scleral region can generate a glaucoma. The demonstration of this important fact, if regarded as the only and essential condition in glaucoma, would certainly be misleading. Indeed, Pagenstecher was able to demonstrate, from pathological preparations, that, in glaucoma, Fontana's space was sometimes obstructed and at other times not, so that this observer was justified in asserting that the occlusion of this space might perhaps influence the height of a glaucomatous process, without necessarily being the cause of the disease. Accepting this view, Mooren reverts to the old theory of a secretion-neurosis, which the investigations of Hippel and Grünhagen support, namely, that every irritation of the trigeminus is followed by increase of tension, through increased secretion at the posterior pole of the eye, which, by pressing forwards the lens and iris, may partially obstruct the filtration channels and interfere with the circulation in this region. These experimenters observed also the regulating influence of the sympathetic in limiting the strong and rapid changes of the blood pressure. Everything, then, points to glaucoma as essentially a secretion neurosis; and, when that fact is kept in mind, the accompanying inflammatory symptoms, whether primary or secondary in character, must, in the main, be regarded as a manifestation of a neurosis, and dependent upon a special condition of the nervous system. In reference to the operations, Mooren regards iridectomy as equivalent to a resection of the nerves, and sclerotomy to a neurotomy. He then disposes of the filtration theory through the cicatrix—a view which has been so far confirmed by the results obtained by Badal, Abadie, and others, who found that, after stretching and tearing out a portion of the external nasal nerve, the tension of the eyeball was reduced and its equilibrium restored.

Cataract.—As regards the pathogenesis of cataract, Mooren quotes with approbation the views of Becker and Michel, which are substantially the same as those enunciated in his clinical observations—namely, that cataract is essentially a secondary affection, and dependent upon states of the circulation generally or within the eyeball. Anæmia, from whatever cause arising, he regards as being the most important factor in the production of cataract, especially of the softer forms; and, when we use the expression, "primary cataract," we do so because we are ignorant of the conditions within the eye which have led up to its formation. This view of the origin of cataract has, in the author's opinion, a very important bearing, not only upon the operative procedure, but also upon the after treatment, to be adopted in individual cases. In the first years of his practice, the old flap operation was the rule, and out of 97 cases 11 were lost; later, when the cases were selected, out of 90 extractions only 6 were lost, thus giving over all a percentage loss of $9\frac{1}{11}$. On the contrary, out of 59 cases with preliminary iridectomy, there were only 2 losses; later, in 170 cases, including a number unfavourable for operation, the loss was 12, and, in a total of 229 cases, the percentage loss was 6·5. As a rule, he prefers Gräfe's scleral section when indicated, reserving his old method for specially unfavourable cases, with this difference that a longer period is allowed to elapse between the iridectomy and the extraction and the replacing the flap by a scleral section. Out of 28 cases—all unfavourable—operated on in 1873, by this modified method, there was no loss, while out of 119 cases operated by von Graefe's method of scleral extraction, with no preliminary iridectomy, 5 eyes were lost, under such circumstances that the non-success could not be referred to a pure accident. These facts, he thinks, speak strongly in favour of preliminary iridectomy, in so far that a possible reactive inflammation of the uveal tract is thus reduced to a minimum. During the last four years, inferior section has been practised in all cases where superior section was inadvisable. In from 50 to 60 such cases there was only one loss; and, while admitting that this good result might be accidental, there can be no doubt that, technically, the operation by inferior section is infinitely easier to perform than extraction by superior section, and therefore less likely to be followed by accident. In occlusion of the pupil following extraction, Mooren prefers De Wecker's operation of iridotomy, and believes that it will always maintain its place in operations for artificial pupil, giving, as it does, a higher percentage of successful cases than iridectomy. In the author's opinion a preliminary, or a combined, iridectomy with the linear section and iridotomy, are the fundamental elements in the operative procedure for cataract, which have reduced the percentage of failures from 10 to 6. The number of failures has been further reduced to about 5 per cent by the introduction of the salicylic compress; for, although the suppurative processes, which so frequently destroy the eye, are not always septic, yet facts prove that septic causes do occasion the loss of the eye in a small percentage of cases. These, then, may be regarded as the matured opinions of the author on the ætiology and treatment of cataract. That the majority of cases are secondary in the same sense as other diseases cannot be doubted, but that the cataractous condition of the lens is always due to some pre-existing morbid process is not so apparent, for if it were so, as the author seems to assume, every case of anæmia, diabetes, chronic Bright's disease, and fluid vitreous, ought sooner or later to be followed by cataract. We know, however, that such a condition of things does not exist, for not more than from 10 to 12 per cent of cases of cataract are complicated with albuminuria—a percentage too small to give it the character of a pathognomonic symptom, and the percentage of cataract in albuminuria is certainly much smaller, and the same will hold for the other diseases.

On the contrary, we know that the lens is endowed with a considerable amount of vitality up to a comparatively late period, and pathological investigation has demonstrated the existence of active processes going on within the lens itself, in connection with cataract, stimulated, it may be, by certain constituents of the surrounding fluids, but not necessarily as a consequence of their presence—*e.g.*, albumen, sugar. The superiority of scleral section over that of the cornea does not seem to be justified in the present case by the statistics, and, moreover, the tendency to an elevated cicatrix, with irregular astigmatism, and the dangerous proximity to the ciliary body, with a possible cyclitis and sympathetic disturbance, must always make this a somewhat risky operation.

The limits of our space do not admit of more than a brief notice of the importance attached by the author to the influence of constitutional states and affections of the nervous system generally, on the optic nerve and retina, and their bearing on prognosis and treatment. Consanguinity, heredity, syphilitic affection of the brain and spinal marrow, eruptive fevers, disturbances of the functions of the skin, anæmia, hæmatemesis, and uterine affections, may each and all, under certain circumstances, be the exciting causes of inflammation of the optic nerve and retina. The illustrative cases, with their appropriate treatment, form one of the most interesting chapters of the work, and will well repay careful study, both to the general practitioner and the pure specialist. The constitutional origin of local diseases is a strong point with the author, and has received more consideration in the work under review than is usually to be found in systematic treatises on eye diseases.

We can most cordially recommend this volume as the outcome of the experience of a singularly acute observer, who, whilst laying no claim to originality, has availed himself largely of the labours of others to give a certain degree of symmetry and completeness to his views on the ætiology and treatment of all the most important diseases of the eye.

The International Encyclopædia of Surgery. Vol. IV. Edited by John Ashhurst, Jun., M.D., Professor of Clinical Surgery in the University of Pennsylvania. London: Macmillan & Co. 1884.

This volume differs in many respects from the three preceding. It is greatly increased in bulk, consisting of upwards of a thousand pages. There are comparatively few contributors, seven in all, three being Englishmen, and the remainder Americans. Excluding the article upon Tumours, it may be said to deal essentially with affections of bones and joints.

The volume opens with a most able article upon Injuries of Bones, by John H. Packard, M.D., Surgeon to the Episcopal Hospital and St. John's Hospital, Philadelphia. One cannot speak too highly of this contribution. It is as scientific in principle as it is practical in application; and the literature of the subject, supplemented by innumerable references, renders the whole all that could well be desired. Reference to a few special points of interest will serve to support these remarks.

As illustrating the care devoted to histology, the section dealing with the repair of fractures may be referred to, where will be found an extensive elaboration of the views held by various authors, to wit those of Paget, Virchow, Klein, Cornil and Ranvier, &c.

In treating of the subject of displacement in cases of fractured clavicle, the author suggests a somewhat ingenious view of the causes producing the assumed position of the fragments. The commonest form of displacement is for the outer fragment to be carried behind the inner, the latter being raised and projected forward. The usual explanation of this is the elevation of the internal fragment by the sterno-mastoid muscle. Against this view, however, Dr. Packard argues that any such elevation is rendered impossible by the rhomboid ligament holding the clavicle to the rib below. His own theory will be best expressed by quoting the text—"Upon the loss of the stay afforded by the unbroken clavicle, the serratus magnus and pectoralis minor muscles pull the scapula forward and inward, while perhaps the rhomboidei muscles draw up its lower angle. The acromion is thus brought nearer to the median line, and tilted downward, the effect of which is to push the distal fragment of the broken clavicle inward, and to depress its outer end. . . . Very possibly the fibres of the subclavius may also draw upon the outer fragment, and aid in the production of the deformity. The forcing of the outer fragment behind the inner will tilt the latter upward and forward."

As illustrating the attention paid to anatomical features, the following point upon the treatment of fractures of the lower end of the humerus may be noticed. A frequent deformity found in these cases is the substitution of an angle salient outward for the normal one salient inward. This results, as Dr. Packard points out, from a "want of recognition of the obliquity of the line of articulation and the application of dressings, no matter in what position—flexed or extended—which press straight across the front of the joint, and thus, by their posterior bearings, push the trochlea upward, and force the upper and inner angle of the lower fragment past the corresponding portion of the upper fragment, whether behind or in front of it." From which consideration, the author indicates that whether the arm be put up in the extended or in the flexed position, the normal angle should be carefully maintained.

The treatment of fractures of the radius is also, for similar reasons, worth noticing. "Two objects are to be especially aimed at: to place and keep the fragments in their normal relation as to their axes, and to maintain their interosseous space." It is to the first of these that Dr. Packard directs particular attention, and indicates the importance of distinguishing between fracture occurring above and below the insertion of the pronator radii teres muscle. Thus, when the fracture occurs above the insertion of this muscle, the upper fragment is rotated outwards by the uncounteracted supinating action of the supinator brevis and the biceps muscles, while the lower fragment is rotated inwards by the pronating influence of the pronator quadratus muscle. "If, on the other hand, the shaft is broken below the insertion of the pronator radii teres, this muscle will draw the upper fragment towards the ulna, but its rotating action will be opposed by the supinator brevis and biceps." In both cases the pronator quadratus will, by its upper fibres, approach the lower fragment to the ulna, while the supinator longus muscle, being principally a flexor of the elbow joint, has but slight influence in causing any rotatory displacement in either class of cases. The practical outcome of this consideration, the author states, is, "that in all fractures above the middle of the bone, the forearm should be supinated, while in all below that point, the semi-prone posture is preferable."

In discussing fractures of the neck of the femur, reference is made and importance attached to a little anatomical point strangely omitted by anatomists in the description of the hip joint. It consists in the almost universal presence of ligamentous bands situated beneath the synovial membrane, and radiating from the head of the bone to the trochanter. Once, in a fresh subject, Dr. Packard states that a vessel of some size was seen by him running along these folds. He is disposed to think that they may exert some influence in diminishing the chance of fracture of the cervix, and in the event of such fracture, such as are unbroken may tend to prevent displacement. Possibly also, vessels borne by them may be of importance in the nutrition of the pelvic fragment. It appears to us, however, somewhat unlikely that these bands, running as they do parallel to the neck, would have any very material influence in checking fractures. That which does not seem at all improbable is, that these fibres, when torn through, get in between the fractured surfaces, and so become one possible cause of the frequency of non-union in this class of cases. Such a suggestion receives some analogous support from what is occasionally found, and perhaps more frequently exists than is generally supposed, in cases of fractured patella, where only weak union is obtained. The rupture of any vessel in these bands would still further check action by depriving the pelvic fragment of blood supply.

Upon the classification of fractures of the neck of the femur there are a few pertinent suggestions; and though these be to alter what are perhaps almost universally accepted and taught as apparently established facts, still the author's claims as a leading authority, if demonstrated by no other proof than this valuable article, justify a full consideration of his proposals and their general adoption if proved after investigation to be correct. The customary classification of these fractures is into intra-capsular and extra-capsular. Now, of the latter Dr. Packard states as his opinion, and this is derived from the examination of a very large number of specimens, that very few examples can be found—that is to say, of simple fractures of the neck outside the capsule, the great majority of such cases being those in which the fracture affects the bone partly beneath the synovial membrane and partly beyond it. He therefore prefers to speak of intra-articular and extra-articular fractures, the latter being those situated entirely outside the capsule, while the former he divides into two classes—one where the fracture passes transversely through the neck, corresponding to the so-called intra-capsular; and the other, in which the line of separation begins close to the trochanter and runs up obliquely and more or less irregularly to the upper surface of the neck near the head, thus corresponding to what is more generally known as the "mixed" form of fracture. The point, of course, is purely of pathological interest. Clinically it is seldom possible to ascertain the exact line of fracture, nor is there any practical advantages to be gained by doing so, seeing the treatment is the same; but for correctness of phraseology the terms intra-articular and extra-articular are certainly preferable to intra-capsular and extra-capsular.

If it is possible to speak more highly of one section than another, perhaps the last, which deals with fractured patella, deserves the highest commendation. The anatomy and treatment are described at great length, and numerous cases given and referred to. The work being in the press before the recent discussions on treatment, necessarily lacks the valuable information which was thus brought forward, but as opposed to the distinctly radical operation of "wiring," the following fact may be referred to and indicates the possible value of pure conservatism in a class of cases where many surgeons at present might be tempted to open the joint and suture the fragments. The particular class of cases alluded to are where there is considerable separation of the fragments, amounting in some cases to an interval of several inches. Illustrations are given showing perfect serviceableness of the limb, although nothing but the simplest means of cure were adopted.

We have noticed at some length this extensive article, occupying as it does more than one-fourth of the whole volume, and yet we can only claim to have cursorily reviewed it. It is to be hoped, however, sufficient has been said to induce surgeons to avail themselves of the valuable information which it contains, and none, we feel sure, will do so without a sense of gratitude towards the author for the extraordinary labour which the compilation of the essay must have entailed.

Diseases of the Joints, by Richard Barwell, F.R.C.S., Surgeon to Charing Cross Hospital, London. Like the preceding article, this is a valuable contribution, as much in the subject matter which it contains as in the ready reference which it permits to collateral literature.

Some little space is devoted to the discussion of Dr. Max Schüller's view of the origin of strumous joints, the author concluding, however, in a non-belief of his infective tubercular theory. In connection with hip joint disease, Mr. Barwell draws attention to a singular coincidence, the frequent occurrence of phimosis with the articular affection. Out of an examination of some hundred cases, it was found that only six had no phimosis. Such a constant association seems to place the fact beyond mere accidental coincidence. The author's explanation is worthy of consideration. "I cannot," he says, "consider this coincidence a mere chance, but think rather that the condition so apt to induce frequent and long continued priapisms has, upon the infantile spinal cord, a deleterious influence, which is reflected back, sometimes in mere awkward or stumbling gate, sometimes more severely in hip disease." However far fetched such an explanation may appear, and Mr. Barwell acknowledges the possibility of adverse criticism as well as many objective and manifold difficulties, still the point is worth looking into; and, should at least the conjunction of the two conditions be as frequent as that indicated in the above statistics, no harm could come of acting upon the explanation given. It opens up the interesting question of the relative frequency of morbus coxarius in Jews and Christians. Should the result of such an investigation tend to support the theory, then we have an additional argument in favour of Mr. Edward Cocks' teaching—though for other reasons—that circumcision should be as universally adopted amongst the Christians as amongst the Jews.

Greater length is perhaps given to the article than really need be, by the introduction of the general symptoms of certain diseases as struma, gout, &c., while the local, from a surgical point of view, would have been amply sufficient.

Excisions and Resections by John Ashurst, Jr., M.D., Professor of Clinical Surgery in the University of Pennsylvania, Philadelphia. Like the author's former contribution in the first volume, upon Amputations, this is a valuable one, and illustrates again his consummate knowledge of his subject, as well as his perfect cognisance of how the material should be best dealt with to be of encyclopœdial value. The article commences with an interesting history of excisions, and then passes on to the description of the special operations. Under the head of each of the major operations will be found some very valuable statistics, rendered the more serviceable by the complete reference attached to each case. It is worth while drawing attention to the table showing the results of excision of the hip joint in cases of hip disease of different duration. A consideration of this table seems to point to delay rather than to haste in operation; that the best time is from nine to eighteen months after the commencement of the affection. The article is profusely illustrated, but the illustrations only passably show the points intended, and otherwise appear very indifferent.

Excision of the Knee Joint, by George Fenwick, M.D., C.M., Professor of Surgery to M'Gill University, Surgeon to the Montreal General Hospital. This short article is devoted to the consideration of a method of operating practised, with very good success in many cases, by the author. It simply consists in removing the disease from the lower end of the femur and the upper end of the tibia, in such a way that a convex surface of the former may fit into a concave surface of the latter. The advantages claimed are less tendency to displacement, and less shortening.

Tumours, by Henry Trentham Butlin, F.R.C.S., Assistant Surgeon to and Demonstrator of Surgery at St. Bartholomew's Hospital, London. Mr. Butlin's reputation as a pathological histologist is a sufficient guarantee that his contribution would be a valuable one. The subject is dealt with extensively and clearly. The most recent theories in tumour origin and dissemination are introduced and discussed, and the article is illustrated by some very good plates of microscopical sections. Some somewhat indifferent chromo-lithographs are introduced, apparently by the editor, as they appear to be taken from patients belonging to the hospital of the University of Pennsylvania. A few special points may be noticed. The section upon nomenclature is good, and we fully endorse Mr. Butlin's wholesome condemnation of such ambiguous and meaningless terms as osteo- or osteoid-sarcoma, osteoid-cancer, osteoid-chondroma, &c. As most of these are sarcomata undergoing either developmental or retrograde changes in the whole or part of their growth, the terms ossefying-, chondrifying-, calcifying sarcomata are far preferable. The endeavour to obliterate the term epithelioma, and classify all carcinomata as spheroidal-, squamous-, or cylindrical-celled, is also to be commended. In the treatment of mammary carcinomata the author ranks with those who advocate the removal of the axillary glands, whether they can be felt enlarged or not.

Injuries of the Back, including those of the Spinal Column, Spinal Membranes, and Spinal Cord, by John A. Liddell, A.M., M.D., late Surgeon to Bellevue Hospital, New York. This is an elaborate exposition of the subject, of great length, due partly to a large amount of needless repetitions, though principally to the introduction of numerous illustrative cases. Hilton's book upon Rest and Pain is freely drawn from, as also Sir William Gull's recorded cases. The subject of pure luxation of the spine—an injury, the possibility of which is still doubted and refuted by many surgeons—is fully discussed, and many cases are given in illustration of the condition. The attempt at reduction of displacements, whether from fracture or dislocation or both combined, is also extensively alluded to, and much given, both in argument and in the shape of recoveries, to support the author's opinion that endeavours to reduce the displacement should be much more frequently made than surgeons have hitherto ventured to make.

Malformations and Diseases of the Spine, by Frederick Treves, F.R.C.S., Assistant-Surgeon to and Senior Demonstrator of Anatomy at, the London Hospital. Under the heading of this article will be found treated the subjects of spina bifida, false spina bifida, congenital sacro-coccygeal tumours, and antero-posterior curvature of the spine, under which latter are discussed cyphosis, lordosis, the rachitic spine, spondylitis deformans, and Pott's disease. The term false spina bifida might with advantage have been discarded. It usually has a very loose and ambiguous application, and to limit it as the author does to true spina bifida, in which the sac is cut off from the canal, is simply applying a misleading name to a stage in the natural process of cure of the true form of the disease.

Handbook of Geographical and Historical Pathology. By Dr. August Hirsch, Professor of Medicine in the University of Berlin. Vol. I—Acute Infective Diseases. Translated from the Second German Edition by Charles Creighton, M.D. London: The New Sydenham Society. 1883.

The first edition of Professor Hirsch's work appeared about twenty years ago, and in 1881 the first volume of the second

German edition was issued from the press. It always appeared to us as very remarkable that the first edition was not translated into the English language, as the work was one of undoubted merit, and the product of an immense amount of labour and critical research; but it is fortunate that the New Sydenham Society has filled up a hiatus in English medical literature by producing, at this early date, a translation of Hirsch's important work.

The general scope of the "Handbook" may be stated best in the author's own words:—"The full aim and object of such an inquiry [historico-pathological and geographico-pathological] is to exhibit the particular circumstances under which diseases have occurred within the several periods of time and at various parts of the globe; to show whether they have been subject to any differences, and of what kind, according to the time and the place; what casual relations exist between the factors of disease acting at particular times and in particular places, on the one hand, and the character of the diseases that have actually occurred, on the other; and finally, to show how these diseases are related to one another in their prevalence through time and through space—a task the high importance of which for the doctrine of special diseases, for etiology and for hygiene, cannot well be misunderstood or called in question.

"The execution of the task demands, obviously, that there shall be a blending of all the points of view previously taken up in historico-pathological and geographico-pathological inquiry; it demands an extension of the view over the whole field of pathology, and a method of handling, whereby the subject investigated may be brought into direct relation with the doctrines of disease."

To accomplish the end in view, Professor Hirsch has availed himself of the labours of all the great investigators in the regions of historical and geographical medicine, and has produced a work peculiarly his own, which must, in all time, form the groundwork of similar and extended investigations.

In the present volume, which presents an elaborate study of the acute infective diseases, Professor Hirsch treats of the more important—viz., Influenza, dengue, sweating sickness and miliary fever, small-pox, measles, scarlet fever, malarial diseases, yellow fever, Asiatic cholera, plague, typhus, relapsing fever and bilious typhoid, and typhoid fever.

The mode by which these subjects are handled may be best stated by an enumeration of the subdivisions of the chapter on Influenza, as follows:—"Always occurs as an epidemic or pandemic—mode of progression—geographical distribution—relation to seasons and weather—special liability of natives and exemption of strangers—influence of the weather in the causation—a specific infection—alleged contagiousness—uniformity of type—coincident outbreaks of influenza among horses—literature of influenza." This chapter is introduced by a chronological survey of epidemics of influenza according to records existing from the years 1173-1875.

In a work of this kind, so wide in its scope as regards space and time, presupposing a critical analysis of the medical literature of the world, it is not reasonable to expect anything like perfect accuracy. In this, the first volume, there are several thousands of references, and an exhaustive analysis of each book and contribution quoted would imply the work of a lifetime of more than ordinary duration. Speaking of his predecessors, Professor Hirsch says, "While one fully recognises the diligence in compilation which runs through many of the above-named works, and the great merits of their authors in arranging and critically sifting the mass of material; and whilst one takes due account of the results to which these investigations have led, yet it is impossible not to feel that they are but the partial labours of pioneers which we may utilise in a measure in our endeavours to reach the goal of all historico-pathological and geographico-pathological inquiry." A similar observation may be made regarding the volume before us—it is a pioneer work; but the work of the greatest pioneer living.

The value of a work of this kind depends on its fulness and accuracy; and this, the second edition, is not merely a reprint of the first, but is virtually a new book. Old matter has been left out, and much new matter has been introduced, bringing up our information to a comparatively recent date, 1880.

As already mentioned, inaccuracies, in a work of this kind, are sure to occur, and we may mention a few of them. In the chapter on Dengue he says:—"It broke out in May, 1824, simultaneously, as would seem, in the province of Gujerat and at Rangoon; somewhat later it was at Chittagong, and in Calcutta during the rainy season (July and August)." Professor Hirsch has evidently derived his information from second-hand references, and not from the original documents; and it is remarkable that every writer on the subject has fallen into the same chronological error. As a matter of fact, the epidemic had attained its *maximum intensity* at Baroda during the last week of May; and there is every reason to suppose that it was introduced into Gujerat in March or April. Dr.

Twining saw his first cases in Calcutta, on 23rd and 24th May, and the disease reached its maximum intensity in June, July, and August. Rangoon was taken by storm on 11th May, 1824; and Dr. Hamilton, under date 20th July, says:—"The type of fever which so generally pervaded the troops during the *latter part of June and the commencement of July* was purely inflammatory, ushered in by more than usual articular pains." Dr. Waddell says:—"The pyrexial epidemic, which visited Calcutta in May, also prevailed at Rangoon in *June* and *July*. It chiefly affected the officers of the army, of whom but few escaped." The disease did not break out simultaneously, in widely remote countries, but had a distinct chronology. Our author falls into similar mistakes regarding the chronology of the recent epidemic of Dengue which had its origin in July, 1870. He says—"Towards the end of the year (November, 1871), it broke out at Bombay, and almost simultaneously in a filthy quarter of Calcutta." Dr. Da Cunha, who is in medical practice in Bombay, saw his first cases in August, 1871; and Bombay was subjected to a second infection in the latter part of December by the troop-ship "Dalhousie." Calcutta was infected in September; and Professor Charles of Calcutta states that "many cases were observed in October among the Jewish community, though the greatest prevalence of the epidemic was not reached till February, March, and April, 1872." Again, in two places, he states, on the authority of Christie, that the disease was unknown on the east coast of Africa previous to the outbreak in 1870; but Christie, in the contribution to which Professor Hirsch refers, says—"The older inhabitants of Zanzibar recognised it as a disease that was epidemic on the east coast of Africa about forty-eight years ago, which was then called Kidinga Pepo. In regard to this, the older people, both Hindees, Arabs, and Suahelis agreed, and their nomenclature was at once adopted."

Probably Professor Hirsch, in the compilation of this great work, availed himself of the services of assistants who were less careful observers than himself; and hence the mistakes into which he has been led.

Regarding typhoid fever, he says, p. 621:—"It was for the most part included with typhus under the comprehensive notion of 'continued fever,' and Jenner, in his classical work, was the first (in Great Britain) to resolve this notion into its elements, and to introduce into England a general *éclaircissement* of the so-called typhoid disease." Jenner's first papers on the subject appeared in the *Medical Times* in 1849-51; but it is well known, though disputed, that the late Dr. Perry, of Glasgow, clearly pointed out the difference between typhoid and typhus fever in 1833. There can be no doubt, however, as to the fact that Dr. A. P. Stewart of London, who was familiar with Dr. Perry's views on the subject, published a paper in 1840, first in Paris and then in Edinburgh, on the differences between the two fevers. We make these observations not in any carping spirit of criticism, but to show that Professor Hirsch is not faultless as regards chronology; and that, when accuracy is essential, the originals must be consulted.

In treating of the etiology of cholera, Professor Hirsch says—"We have become so used to the notion of the cholera poison multiplying within the bodies of the sick, and of the choleraic discharges in particular being the carriers of the same, that we hardly appreciate now-a-days how slight a foundation there is for that opinion." Again (p. 492)—"There are many well-known observations which make it probable, at least, that some such connection subsists between the occurrence of cholera and the use of infected drinking water; but any exact proof that the connection is a direct one is not forthcoming, for the present at any rate, inasmuch as it is always open to us to contend that the drinking water may have been fouled with excrementitious matters, especially the alvine dejecta, that it does not exert a *specific* action, but acts injuriously in a general way through the putrid matter that it contains, and is calculated merely to heighten the predisposition of the individual for the specific attack of sickness." At the conclusion of the paragraph from which this quotation is made, he says—"The specifically injurious action of a sample of drinking water can only be inferred if exact proofs are forthcoming that cholera sickness has been caused by partaking of it." There is, or may be, a little indefiniteness as to the definition of "exact proofs"; but our author does not apply this crucial test to Pettenkofer's theory as to the "*significance of the height and fluctuations of the sub-soil water for the production and diffusion of the acute infectious diseases, cholera in particular.*"

We hope that Professor Hirsch may be spared to produce a third edition of his work, at a much earlier date than twenty years hence; and we venture to predict that his views on the etiology of cholera and enteric fever, in particular, will by that time have undergone a considerable change.

We cannot but express, in conclusion, our appreciation of the labour of Dr. Creighton in translating the work before us.

Few would, or could, have undertaken such a task; and, in so far as we can judge, it is a faithful reproduction of the original. We cannot, however, accord the same praise to the readers of the proof sheets; for we have seldom perused a work so full of glaring blunders from beginning to end, more especially as regards proper names. Blunders of this description are, however, by no means unmitigated evils; for we are often, through the medium of such blunders, enabled to detect the sources from which authors have compiled their so-called original material.

Annals of Cholera, from the earliest periods to the year 1817, By JOHN MACPHERSON, M.D., Inspector-General of Hospitals, H.M. Bengal Army. London: H. K. Lewis. 1884.

IT is no easy matter to write the annals of any disease from the earliest period till recent times; for a complete record of this kind implies a thorough knowledge of the medical literature of all times, and of every country. The first edition of the "Annals" appeared in 1872; and this, the second edition, is merely a reprint of the first, with an appendix on "The Analogies of Cholera nostras, and Cholera Indica," and a note on "Endemic and Epidemic Colic." Our author states that, since the appearance of the original edition, he has not been searching for any new facts; and the only addition made is the record of an epidemic which, on the authority of Jehan Gir, broke out in many parts of Hindostan in 1616. He hesitates, however, to pronounce that this plague was cholera, "because the word *waba*, although used by Arabs at this day for cholera, is not so specific a word as *haiza*." The oldest specific name, in Arabic, is certainly *haiza* or *haidsa;* but, in popular language, it is also termed *taoun* and *waba*. In a supplementary note, the additional Arabic designations, *Maradi ul aswad* and *Halgi Bhaka* are given; but *Rih-el-asfar*, "*The Yellow wind*" is not mentioned.

Since the appearance of the first edition, in 1872, some substantial additions have been made to the early literature of cholera in the empire of China; or we might rather say that, in respect to China, what was formerly regarded as conjectural, is now brought more clearly within the realm of certainty. Dr. Dudgeon, of Pekin, who is undoubtedly the highest authority on everything relating to Chinese medical literature, says, "Cholera has been known in China, as in India, from time immemorial. It was described, 2,500 B.C., by the very name which it now bears, viz., *hwo-luan*, an expression meaning something huddled up in a confused manner inside the body, and which is evidenced by the vomiting and purging. No mention is made by any of the numerous authors I have consulted as to its epidemic character, which has characterised the visitations of this affection in India, and latterly in Europe, since 1817." In all early records there is necessarily an element of uncertainty; but there is every reason to suppose that the Chinese, from the earliest historic times, were acquainted with the disease. They divided cholera into two kinds—the wet and the dry. The latter form, which was considered the most fatal, was characterised by the absence of vomiting and purging. According to Dr. Dudgeon, the disease has been described by various Chinese authors from 221 A.D., till 1644 A.D.; and this brings us down to the time of Cleyer, who wrote in 1669.

Dr. Macpherson does not ignore the Chinese annals; but he entertains doubts as to their veracity. This, however, is a point which can be determined only by Chinese scholars such as Dr. Dudgeon.

The third chapter of the "Annals," which traces cholera from the earliest ages to the year 1500, is exhaustive and full of interest, quotations from the different authors being given, so that the reader is left to the exercise of his own judgment.

In chapter four, cholera is traced, in the West, from A.D. 1500 to A.D. 1817; and here we enter on debatable ground. Much confusion exists, from the fact that the various terms used to designate cholera are also used to designate other diseases; so that we have to rely solely on the account given of the symptoms. Thus, in French medical literature, we have numerous references to outbreaks of a disease called *trousse galant*, a popular designation for cholera at the present day; but it is very evident that more than one disease is comprehended under the term. In 1643, Van Der Heyden wrote as follows—"The furious onset of *trousse-galant* in a short time takes away from the body so much of its substance and its force, and occasions in it so much mischief and change, that in seven hours their domestics would not recognise in such a sufferer a master or a relative, unless they knew it could be none else; for they encounter the true Hippocratic expression which indicates the extreme of debility and the image of death. Once, when I was called to see a patient, only five hours after his attack, I found him in a condition giving the most unfavourable prognosis, to wit, without pulse or speech, passing in his evacuations only a fluid resembling clear milk. Along with this, his eyes were so sunk that one could scarcely see them; and his legs and arms so drawn back by convulsions that one saw no movement in them; and so cold from the moisture of a cold and clammy perspiration adhering to them, that the patient seemed more dead than alive."

There can be no doubt whatever in the mind of any one who has seen cholera in the East that Van Der Heyden's patient suffered from true cholera.

It is probable that, about this period, there was a widespread diffusion of cholera in Europe. It was epidemic in Ghent in 1665, and it prevailed in England from 1669 till 1672, or 1676. Sydenham, in 1676 and 1680, described two forms of cholera, differing from each other in every essential respect; and it is very evident, from a detail of symptoms, that he was acquainted with true Asiatic cholera. In 1649, Rivierus, of Montpellier, in describing an epidemic of contagious dysentery, which may have been an epidemic of cholera, states a case in which all the members of a family got it from the use of common latrines. Throughout the seventeenth century cholera seems to have been a common sporadic disease in Europe; and, during the first half of the century, it was especially so in France and Belgium; and, in the second half of the century, in Great Britain, and especially in London. None of the European epidemics of this period seem to have equalled the epidemics of India, either in malignity or extent. In 1768-69, an epidemic on the Coromandel coast carried off 60,000 victims in one year; and, shortly after, among the pilgrims collected at Hurdwar, 20,000 are said to have died in less than eight days.

The historical sketch given by Dr. Macpherson is full of interest; and affords, at least, an extensive basis for those who may wish to investigate still more minutely the various epidemics of cholera which have occurred within historic times, till 1817, the date of the first epidemic. In the general summary, our author says—"No disease appeared in India in 1817 that had not often appeared there before—no symptom manifested itself that had not often been witnessed before—no new habits of the disease were developed. The natives of India invented no new name, and worshipped no new goddess, for the disease of 1817. A pretty sure sign that they did not think the malady a new one. Two new attributes, however, have been sometimes ascribed to the disease of 1817, contagiousness and power of spreading. As regards the first, there is no reason to believe that the disease in that year was either more or less contagious than in similar outbreaks in former years. As regards the second, the disease had often spread widely before; and it can at most be said that this power of spreading was intensified."

It is somewhat remarkable that the prevailing opinion, at least among medical men, seems to have been that cholera was neither infectious nor contagious; but this belief seems to have been founded solely on negative evidence. Cholera epidemics were studied *en masse,* and not in detail: and scarcely a single attempt seems to have been made to analyse the constituent parts of an epidemic. In the "Annals," however, we have a valuable collection of facts which are undoubtedly, in many instances, the fragments of wide-spread epidemics which future investigators may succeed in jointing together. The recent epidemic of cholera in Egypt has had the effect of stimulating scientific research: and, when the etiology of the disease has been placed on a scientific basis, we may be enabled to unravel the tangled skein of cholera annals. It will be necessary, also, in order to elucidate the mysteries of cholera epidemics, to study the manners and customs of the people, and the movements of the population along the highways and byways of human intercourse at different seasons of the year.

The Annals of Cholera is the standard work on the subject in the English language, and the medical profession is indebted to Dr. Macpherson for the issue of this, the second edition of his work, which we commend to the notice of our readers.

Asiatic Cholera: being a Report on an Outbreak of Epidemic Cholera at a Camp near Murree in India. By C. M. Jessop, M.R.C.P., Brigade Surgeon H.M.'s. British Forces. London: H. K. Lewis, 1883. P. 47.

Dr. Jessop, in the pamphlet before us, analyses an outbreak of cholera which occurred at Camp Gharial in August, 1876. The troops encamped at Gharial on 1st August numbered 458 Europeans, including officers, men, women, and children. On the same date, there were in the bazaar 339 natives. On the 24th July, cholera attacked the troops at Murree, twenty-six cases having occurred amongst the Europeans. The number of cases amongst the natives was unknown. On the 28th July, a detachment of troops from Murree moved into camp on Topah, a distance of two miles, the tents being within sight of camp Gharial, and 1,600 yards distant.

Dr. Jessop connects the outbreak at camp Gharial with the previous outbreak at Murree, or with the detachment which moved to Topah on the 28th July, and there is *primâ facie* evidence that this was the case, as the only cases of cholera which occurred in the vicinity of camp Gharial were those in the camp at Topah. The statement is made that "there were no local insanitary conditions that could be detected to favour the spread of epidemic disease," and that "there is no evidence that any cases of cholera arose from water polluted with cholera discharges." There was, however, direct and indirect communication with the bazaar and commissariat at Murree, where cholera was prevalent.

From a table given, we learn that, on 30th July, a native sweeper was attacked with *English cholera,* and died on 2nd August. On 1st August another native sweeper was attacked with *English cholera,* and died on the same day. On 3rd August, a woman was attacked with *diarrhœa,* and recovered on 31st August. On 17th July, a woman was attacked with *dysentery,* and died on the 7th August. On 6th August, a private was attacked with *diarrhœa,* and recovered on 27th September. We have thus, between 30th July and 6th August, five cases of diarrhœal disease and three deaths.

On 7th August, a woman was attacked with *cholera,* and died on 9th August. From that date till 31st August, there were 18 cases of cholera and 2 of diarrhœa.

Regarding the first and second cases, Dr. Jessop says:—"Both these cases were regarded as *English cholera,* because it had been reported that they had eaten of putrid pig's flesh instead of burying it. The symptoms of the first case certainly lend considerable support to this view. It was, however, found not to be true; nevertheless, it was a useful *canard* at the time, for it allayed the excitement among both natives and Europeans." Again, at page 40, he says:—"On 30th July, and subsequently, five suspicious cases occurred. These cases were all treated as if of an infectious character; no disease spread from them." This certainly is a very handy method of arriving at a sweeping conclusion. Number one had been greatly purged all the day previous to his admission to hospital. Number two had been seen vomiting near the cook house; and he had been greatly purged. Number three had been purged for five days. Number four "was treated for acute dysentery affecting the whole of the colon;" and number five had been purged for two days prior to admission. From Dr. Jessop's own account of the symptoms, these five cases were, to say the least of it, very suspicious; yet he disposes of them summarily by saying "no disease spread from them."

Dr. Jessop has a theory, we might say a pet theory, regarding the propagation of cholera; and we may give it in his own words—"On 7th August, five tinmen were found in camp, strangers from Murree, and though they remained healthy, yet it is possible that they may have been direct carriers of 'cholera air' from Murree bazaar by their clothes —especially the pugrees (turbans). This air, mingled with air already in a condition to receive any fresh conditions for evil, may have been the cause of the outbreak. These men tinned the cooking utensils of F company, and they would be naturally in close relationship with the eight cooks of that company, who may have disseminated some 'unwholesome air' to the various tents to which they conveyed the messes." It happened, however, that cases occurred simultaneously in E company, and our author says:—"It must, therefore, be assumed that the disorder, however introduced, tainted the air of the camp generally, and set upon such persons as were in themselves in an insanitary condition, irrespective of their neighbourhood." He seems to be rather suspicious, after all, about the "cholera air" in the pugrees of the five tinmen; for he says, page 31—"Of course, the tinmen may have had everything to do with the cholera in camp, still there were the five suspicious cases preceding their arrival, and as three of these sick persons had died, it is hard to believe the disorder was not brought direct by wind from Topah, only 1,600 yards distant." Because three men died, probably from cholera, it is hard to believe that the disease was not carried by the wind! The wind theory is so flexible that it can be adapted in the most satisfactory manner to any case. By way of accounting for some cases which occurred after 21st August, he says—"Before this date all cholera had ceased on Topah: cases still occurred in Murree bazaar, and as the direction of the wind was from the S.W., it is possible that 'cholera air' may have then blown from Murree, &c., into valleys to the eastward of Topah and Gharial which, hanging on the sides of the hills, was dislodged by the powerful easterly breeze of 21st August, and sent clear of Murree in the direction of Peshawur."

We shall not follow our author farther into the region of epidemiology. Dr. Jessop has very strong opinions also regarding the treatment of cholera; and we believe that the laudable object which he had in view in submitting his book to the profession was that his mode of treatment might receive an extensive trial.

Eighteen cases of cholera were treated, and of these twelve died and six recovered. Of the twelve who died, five were treated by chloral hydrate, and seven were not. Of the six who recovered, "five were treated more or less with that drug, and recovered by change from the horizontal to the vertical position." "In the cases in which this method was employed there was no resilient or restorative power; cerebral congestion was present in all—all would have died had not semi-reclination or the sitting posture been adopted to alter the heart's motion." In explanation of this mode of treatment, he says,—"In the horizontal position, the intrinsic or erectile movement of the heart decreases; there is a minimum frequency with a maximum strength of pulse; there is diminished respiratory motion in consequence of a diminished respiratory rate. In the sitting or vertical posture, the frequency of the pulse is increased, while its force is lessened; the number of respirations are increased, more air is circulated in the lungs, hypostatic congestion is decreased; the suction power of the auricles is increased; this unloads the brain and the lungs, improves the systemic circulation, and counteracts the effect of gravity by using the means at disposal." As might be anticipated, this change of position is not agreeable to the patient, for "he is not disposed to use it; he does his best to prevent it, and cries out at the change; his tendency is to get his head as low as he can, or even to hang it down."

From a careful perusal of Dr. Jessop's book, we cannot but come to the conclusion that he is not endowed to any great extent with the detective faculty; and that his experience of cholera patients is not sufficiently extensive to admit of his speaking with authority on the subject.

MEETINGS OF SOCIETIES.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

Session 1883-84.

Meeting X.—*25th March, 1884.*

Dr. Alexander Robertson *in the Chair.*

Dr. Newman showed a case of movable kidney upon which the operation of nephroraphy was performed, on the 29th March, 1883. The patient was aged 40 years, and has had seven children and three miscarriages in twelve years. A swelling was detected for the first time after her sixth confinement, but she did not suffer much inconvenience from it until the period of her seventh pregnancy, when she complained of severe pain and dyspeptic symptoms.

The movable tumour was discovered before, but its real nature was not ascertained until after her seventh delivery. The symptoms were always aggravated during the menstrual periods, when she complained of a burning pain in the epigastrium, with severe and sometimes persistent vomiting, sickness, and occasionally diarrhœa.

For two years previous to the operation she had been unable to get out of bed, but since then she has been able to go about her usual household occupations, and even walk a couple of miles. Previous to the operation, the following were the physical signs:—Pendulous abdomen; the right kidney was found to be freely movable, and on account of the thinness and flaccidity of the abdominal walls, it could easily be grasped by the hand, and its form made out. The left kidney was also found to be movable, but not to the same extent as the right. When the kidney was displaced by thrusting the hand into the loin, there was a sense of diminished resistance in the lumbar region, and percussion was slightly more resonant than normal. The right kidney was easily moved up under the ribs, downwards into the pelvis, and across the middle to about two inches to the left of the middle line. As no permanent relief could be afforded by medical treatment, or the application of an elastic bandage to the abdomen, it was resolved to perform the operation of nephroraphy, which was done by making an incision in the right loin, immediately to the outer edge of the quadratus lumborum. The kidney was exposed, its capsule opened and stitched to the edges of the wound in the parietes, while three cat-gut sutures were passed through the cortex of the kidney and the abdominal walls. A drainage tube was inserted to keep the deep edges of the wound apart, and so allow the formation of a considerable bulk of granulation tissue.

In about a month after the operation, the patient was able to move about, and since then she has improved in her general condition, and has been able to walk about in comparative comfort. The left kidney, however, is still movable, and for it she has been compelled to wear an elastic bandage and air pad; but the right kidney has never troubled her since the operation was performed. She has now come to town to see if anything can be done to fix the left kidney, as it has been troubling her more than formerly, and the movements are a good deal freer than they were a year ago. The right kidney can easily be felt adhering to the deep edges of the wound in the loin, and is not now capable of any movement.

Dr. Newman thought it was a proof of the efficiency of the operation, that until now the patient had never complained of any symptoms which she formerly referred to movement of the kidney on the right side. For ten months subsequent to the operation, the elastic bandage and air-pad formed a sufficient support to retain the left kidney in its normal position, but since the end of January last it had become insufficient to control the movements. The question, therefore, has arisen whether or not a second operation should be performed.

Dr. Newman stated, that since last year he had seven cases of movable kidney under treatment, and in all of these the application of the elastic bandage had so ameliorated the symptoms that operative interference was uncalled for, although before the bandage was used some of the patients were urgent for an operation to be performed. Few cases, indeed, require the performance of nephroraphy, but when the symptoms are very severe and persistent, and the patient is unable to control the movements by the use of a bandage, then an operation should be performed.

The method of employing pressure which Dr. Newman has found most useful, and most easily applied by the patient, is to have a well fitting abdominal elastic bandage, extending from the line of Poupart's ligament to the level of the sixth or seventh rib. The bandage should be made to fit the body accurately and firmly, but without exerting undue pressure at any point. It may be made of one piece, or, what is much better, of strips of elastic bandage sewn together, and united in the middle line in front by means of steel slips similar to those used to fix stays. The patient should have her bowels well opened every morning, and then, before getting up for the day, should slip over the lower extremities, and upwards around the abdomen, a tight-fitting jersey, applying over the region of the kidney an air pad, and then buckling over it the broad elastic bandage.

The air pad is made so that it may collapse while the patient is putting on the bandage, and after the bandage has been adjusted, the pad is inflated by means of a tube carried to the mouth, and provided with a stop-cock. These pads have been made by Mr. Hilliard, who can supply them to any who may wish them. It is a matter of great importance that the elastic bandage should extend from the level of the sixth or seventh rib to Poupart's ligament, so as to thoroughly support the abdomen, otherwise it is almost certain to prove worse than useless.

Dr. Macewen presented a boy from whom the right kidney had been removed about ten weeks previously, on account of suppuration of that organ, which had extended so far as to have virtually destroyed its excretory function. He had previously suffered for many months from symptoms of renal calculus. When admitted under Dr. Macewen's care he was in a semi-delirious and very exhausted condition, with flickering, feeble pulse and abnormal temperature, lividity of face and lips. On examining the small quantity of urine obtained on admission, it was seen to have a copious deposit of pus. On microscopic examination tube casts containing pus corpuscles were found, besides numerous crystals of phosphate of lime. There was an increased area of renal dulness on the right side, and a marked fulness in the lumbar region. He was at once operated on by lumbar incision. After removing a large quantity of fœtid pus (swarming with bacteria) and shreds of sloughing cellular tissue, two renal calculi were detected in the kidney, one of which occluded the ureter and occupied the greater part of the pelvis, the other in the substance of the organ. Both were removed, but, on further examination, the whole organ was found to be filled with abscesses, leaving little or no excretory surface. It was therefore removed. The boy was then placed in the hands of careful nurses, who watched him with the greatest care. After recovering from the shock of the operation, he progressed with astonishing rapidity. The boy, who was reduced to a mere skeleton, became, at the end of a month, so fat and plump that those who had seen him at the operation had difficulty in recognising him in his altered state.

The boy was then shown, looking healthy and stout, with his wound quite healed.

Dr. Alexander Robertson had heard an account of the case before, and regarded it as one of very great success. A patient with only one kidney naturally ran greater risks of fatal renal disease than one in whom both organs were present, and the only doubtful point about the present case is that there is still some albumen in the urine.

Dr. James Dunlop expressed the pleasure he had in seeing the case, and congratulated Dr. Macewen upon his result.

Dr. Thomas testified to the great improvement in the boy's health since the operation.

Dr. Newman thought that the hypertrophy going on in the remaining kidney might account for the albuminuria by the fact of increased blood supply being present; but in this case it might be connected in some way with the attack of erythema from which the boy had suffered.

Dr. A. Fuller showed, for Mr. Clark, a patient with a movable tumour in the right side of the abdomen, which might be a kidney, and read the following notes of the case:—

Mary B——, aged 37, was admitted to the Royal Infirmary in December last, complaining of a tumour in the abdomen. The patient has been married for nineteen years, and her husband is still alive; she has always been in very good health. She has had ten children, the youngest of whom is four years old. Her labours, pregnancies, and puerperia were all normal. Two years ago she miscarried at the fifth month, which was accounted for by a strain she got when lifting one of her children. Six months later she again miscarried at the third month, and six months ago she had a third miscarriage, also at the third month; she was able to assign no reason for these latter miscarriages. Since the last one, menstruation has been very irregular, the menstrual flow coming sometimes once in six weeks and sometimes twice a month; it generally lasts for two or three days.

It was after her recovery from her first miscarriage, nearly two years ago, that she for the first time noticed the tumour which she now complains of. It was situated on the right side below the margin of the ribs, and was, she thinks, not quite so large at that time as it is at present. It gave her very little trouble, and until twelve months ago she never noticed it to be at all movable: but for the last twelve months it has gradually become more so. It has given her very little trouble, but when she lies on her left side she experiences a dragging pain in it which sometimes makes her feel sick. For two or three weeks before her admission her face used to be a little swollen in the mornings, and she complained of not seeing distinctly, her vision being somewhat misty. Her feet were also slightly swollen, both before and after admission. She has noticed for some time that on certain days she passes a good deal of water. Occasionally she has some difficulty in making her water, but this is not coincident with menstruation.

Present condition, 25th March.—The patient is a well-nourished healthy looking woman. For a week after her admission there was some slight puffiness of the face and dropsy of the lower limbs, but this has now completely disappeared.

She has a good appetite, but is occasionally a little sick in the mornings, and often feels a good deal of nausea before breakfast. She complains of pains in her back, not very well localised, but in the lumbar region, not increased at menstruation. The bowels are regular and motions natural.

Examination of the abdomen.—The patient lying on her back. The abdomen is somewhat prominent and pendulous, and is well marked with *striæ gravidarum*. Pulsation is visible along the line of the aorta.

In palpation the abdominal walls are found to be extremely flaccid. There is no tenderness on gentle palpation over the abdomen. A tumour can be felt, somewhat oval in shape, with its long diameter directed obliquely downwards and outwards, projecting about 2½ inches below the lower border of the ribs, and with its inner border touching the anterior mammary line at the lower border of the ribs. It feels about the size of a large goose's egg, and has a tolerably firm consistence. On grasping the tumour, or pressing it firmly, no pulsation can be felt, but the patient complains of a throbbing pain in it. No pulsation can be detected going to the tumour. Its lower border is felt as very distinct and rounded, but its upper border is less defined and, in ordinary respiration, is almost under cover of the ribs. With forced inspiration, the tumour descends vertically, and its lower border can be felt on a level at least 1 inch below the umbilicus, its descent being about 3 inches. The liver percussion dulness is also found to descend for about the same distance during forced inspiration. The tumour is very freely movable in all directions, and may be pushed back into the right lumbar region. It always tends to return to its position, as described, however, when the patient is on her back. When lying well over on the left side, it comes forwards and downwards towards the umbilicus, its left border being about 1 inch to the left of the middle line, and its lower border about ½-inch below the umbilicus. With the patient on her back, only a slight impairment in the clearness of the percussion note is to be detected, except about ½-¾ inch below the lower margin of the ribs where the percussion note is dull, this dulness being continuous with the liver dulness. The liver dulness is normal, begins to be impaired in fourth interspace, and ends just below the ribs, 5 inches.

With the patient on her face, the percussion note, in the interval between the lower margin of the ribs and the iliac crests, is on the right side quite clear at a distance of 2 inches outwards from the lumbar spines. On the left side, at a corresponding point, percussion is absolutely dull, and the dulness continues outwards for 2 or 3 inches beyond this. There is no marked absence of fulness to be detected on the right side either with the eye or hand.

After her admission the urine was occasionally examined for albumen, which was found in slight traces in two or three specimens. No tube casts or other evidence of kidney disease was found microscopically. A note of the quantity of urine *per diem* was not kept. In a specimen examined to-day, no albumen was found. The spleen was percussed, and found to be normal in size and position.

A vaginal examination was made, and the uterus found bimanually to be to the front. The anterior vaginal wall was extremely flaccid and somewhat prolapsed. There is hardly any perinæum.

In the circulatory system there is a soft systolic murmur to be heard at the apex, and she complains of slight shortness of breath, but nothing more.

The other systems are normal as far as ascertained. About six weeks ago patient was dismissed, wearing an elastic abdominal jacket with an air pad fixed on the inside over the position of the tumour. She somewhat objects to the jacket as uncomfortable and causing a throbbing in the tumour.

Dr. Francis Henderson said the tumour was hard, superficial, freely movable, and, if a kidney, would, he thought, come under Dr. Newman's definition of "floating." Although it was difficult to make up one's mind after so rapid an examination, yet he thought that the tumour in the present case might be a kidney.

Dr. Newman said that Mr. Clark had kindly asked him to see this case when she was in the hospital for the first time, and he thought, taking all things into consideration, the probability was the tumour was not likely to be the kidney, from the fact that the centre of rotation was not like that found in cases of movable kidney. The tumour, at the time it was first seen by Dr. Newman, gave rise to a decided dulness on percussion anteriorly, and followed the movements of the liver during respiration, and the tumour did not give rise to that peculiar fainting sensation which is so characteristic of a movable kidney. Undoubtedly the form of the tumour would lead one to suspect it to be a kidney, but while it is freely movable in all directions, and may be pushed back into the loin, yet the tendency for it to replace itself in its former position, close to the lower margin of the liver, would lead one to suspect that it had some connection with the liver.

Dr. Newman said that he had never seen a case of movable kidney where it had given rise to a dull percussion in front of the tumour, and the reason of this is easily understood, when it is remembered that the kidney moves about, not in the peritoneal cavity, but posterior to it, thus being separated from the anterior wall of the abdomen by the small intestine.

The point round which the tumour appeared to move corresponded to the notch in the anterior margin of the liver, situated above the gall bladder, and therefore would lead one to suspect it to be either connected with, or developed from the gall bladder, and the circumstance that the dull percussion over the tumour is continuous with the hepatic dulness, seems to support this view.

Dr. J. Lindsay Steven said that the chief point, so far as he could judge, in favour of its being a tumour other than the kidney, and probably connected with the loin, was the very marked and decided descent of the tumour on a deep inspiration. But in favour of its being a kidney, there were those points—viz., lying on her left side, the tumour could be felt at the umbilicus; there was clear percussion behind, over the normal site of the organ; and the shape, so far as it could be felt, was not unlike that of the lower end of the kidney. It was very difficult (with the present limited means of examination) to be sure, but he thought it not unlikely that the condition might be due to displacement of the kidney.

Dr. James Dunlop showed a leg which had been amputated on account of a sarcomatous tumour connected with the tibia and fibula, and read the following notes:—

James M., æt 17 years, a salesman, was admitted into the Glasgow Royal Infirmary on the 22nd November, 1883, and was dismissed cured on the 12th January, 1884.

The history of the case was as follows—About eight months previous to admission, a small oval swelling presented itself in the upper third of the right leg; it was painless.

It gradually increased in size, but continued to be free of pain. His health was not impaired. and he was able to attend to business.

In the course of four or five months it had attained the size of a man's clenched fist. It was at this period of its growth cut into and partially removed by a surgeon outside the hospital. The open wound left never healed. It continued as a raw surface, rapidly increasing in size up till the present date, when it now occupies an area of four or five square inches.

It presents a fungoid appearance; a portion of the tumour in the centre being hollowed out. This hollowed out portion is surrounded at its circumference by a large, soft, flabby mass of red, raw looking granulation, which bled readily, but was painless to the touch.

Upon pressing the probe into the exposed portion of the front of the tibia, a sponge-like elastic resistance is experienced by the hand, the probe sinking to a considerable depth in the softened and diseased bone from the raw surface.

The discharge of blood had been so copious, that the patient presented a weakly and pale appearance.

There was no history of a local exciting cause, no family history suggestive of hereditary transmission, and there was no lymphatic complication.

Amputation in the thigh was followed by a rapid improvement in the general health; and the patient was dismissed on the 12th January with a satisfactory stump.

In connection with this case, Dr. Dunlop made some remarks on the best form of stump for the application of artificial limbs. Generally speaking, we had three forms of stump in amputation immediately above the knee—(1) where, in children, the epiphysis was removed, here the stump was round; (2) the Carden, where the stump is flat; (3) where the limb is removed above the condyles, here the stump is conical. From experience he had found that the best form of stump for an artificial limb was the conical; and lately he had removed an additional piece from the femur of a gentleman for no other reason than to get a better stump for an artificial limb.

Dr. Newman said the naked-eye appearances had been so well described by Dr. Dunlop in his clinical report that it was unnecessary to describe them here. He wished, however, to call attention to the microscopic appearances as illustrated by the specimens on the table. The vessels in the tumour were injected with carmine from the popliteal artery. The tumour is composed entirely of spindle shaped cells without any distinct stroma, but embedded in a homogeneous matrix which is tolerably abundant in the central portions of the tumour, while in the periphery the cells lie in very close contact, and the matrix is hardly visible.

The specimens show remarkably well the vascular arrangement in a tumour of this description. The capillaries are seen to run between the cells forming the mass of the tumour, without having anything like a vessel-wall. The only difference in the cellular arrangement being that around the lumen of the capillaries, the spindle cells have somewhat of a concentric arrangement, the same as what is seen in newly developed blood-vessels in granulation tissue.

The tumour is evidently a large spindle-celled sarcoma. These tumours (the large spindle-celled sarcomata) differ from the recurrent fibroid tumours of Paget, in so far that they are more rapid in growth, more malignant in their character, and their cellular elements are more abundant and more closely related to round-celled tissue, while, on the other hand, they differ clinically from the cancers, in that they usually occur before the age of thirty-five, are not hereditary in their nature, and their mode of infection is by the blood-vessels rather than by the lymphatic system.

The lymphatic glands are, therefore, not so frequently involved in cases of sarcomatous tumours as in cancers.

The histological structure also is very different. The sarcomata are essentially cellular, and formed on the type of embryonic connective tissue, there is no distinct stroma, and the blood-vessels pass between the cells, there being no proper vessel wall. In the cancers the stroma is more or less abundant, and distinctly separated from the cellular elements which are essentially epithelial in their origin. The blood-vessels have well formed walls, and occupy the stroma, do not pass between the groups of cells, and whereas cancers are notoriously hereditary, the sarcomata are not so.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Examination of Urinary Sediments.—"Certain urines, especially concentrated urines and those passed in febrile conditions, deposit, upon standing, a considerable sediment, composed chiefly of alkaline urates, often deeply coloured by uroerythrine. The detection of the organised sediments, such as blood globules, epithelium, casts, &c., in such cases, is difficult, and often impossible. To facilitate the examination in such cases, Méhu (*Journ. de Pharmacie et de Chimie*, 1883, p. 228) adds to such sediments, after they have subsided, and after most of the urine has been removed, a small quantity of a saturated aqueous solution of ordinary sodic phosphate. This dissolves completely the pigments and urates, so that the subsequent examination of the sediment is comparatively easy. An excess of the sodic phosphate does no harm, but its addition frequently causes a precipitation of crystalline calcic phosphate. Méhu prefers this method of treatment to the common one, in which the urates are dissolved with water. The latter delays the subsidence of the organised elements, and besides, tends to soften and break them up."—*Boston Med. and Surg. Journal.* 7th Feb., 1884.—D. M'P.

Curability and Treatment of Locomotor Ataxy.—Among 300 cases which Eulenburg has been able to follow, he has found only three cures. He believes, however, that the number might be increased were sufficient energy expended on the treatment. He holds that the curative action of silver is incontestable, but that it is often inert when given in the form of pill or powder. He recommends that it should be given subcutaneously, either as the albuminate or as the hyposulphite:—

℞	Chloride of silver, . .	10 centigr.	
	Hyposulphite of sodium, .	60 ,,	
	Distilled water, . . .	20 grammes.	M.

An injection is given daily in the dorsal region, of 10 centigrammes to 1 gramme. There is generally a temporary disappearance of the pains, and when they reappear after two or three hours they are generally removable by a cold compress.

Hypodermic injections of strychnine in doses of 4 to 6 milligrames have, in several cases, been followed by a remarkable diminution of the motor and sensory disorders. Local refrigeration by means of ice or cold compresses along the vertebral column has had beneficial results. The degree of cold must be determined by the individual sensibility of the patient. With the nitrate of silver, the continuous current, and local cooling, he has in numerous cases improved the patient's condition.—*La France Médicale.* 10th January, 1884.—G. S. M.

"Paget's Disease:" question of priority.—Dr. Walter F. Atlee, in the April number of the *American Journal of the Medical Sciences*, calls attention to the diagnosis of Paget's disease of the nipple, and claims that Nélaton described the affection some twenty years prior to the appearance of Sir James Paget's paper on the subject. He quotes a case, resembling "Paget's disease" in many respects, from a volume entitled "*Clinical Lectures on Surgery*," by M. Nélaton, published in 1855.

A new method of producing Anæsthesia.—We hear from Lyons of a new mode of producing anæsthesia, but it seems to have been practised previously in Copenhagen. It consists in introducing ether by way of the rectum. An india-rubber tube, about the thickness of one's finger, is introduced into the rectum, and by its other end connected with a bottle of ether which is placed in a water bath of the temperature of 50° C. The ether boils, and its vapours pass into the intestine, where they are quickly absorbed by the mucous membrane. In about five minutes, the breath of the patient gives off an odour of ether, and anæsthesia quickly follows, without being preceded by a period of excitement. The return to consciousness is effected calmly, and there is no vomiting. The quantity of ether given may, by this method, be most accurately determined.—*Gazette Médicale de Paris*. 5th April, 1884.—J. Y. M.

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On Bedside Urine Testing, including Quantitative Albumen and Sugar. By George Oliver, M.D. Second Edition. London: H. K. Lewis. 1884.

The Parent's Medical Note-book. Compiled by A. Dunbar Walker, M.D., C.M. London: H. K. Lewis. 1884.

Medical Fashions in the Nineteenth Century, including a Sketch of Bacterio-mania and the Battle of the Bacilli. By Edward T. Tibbits, M.D. London: H. K. Lewis. 1884.

The Medical Annual and Practitioners' Index: a Yearly Record of useful information on Subjects relating to the Medical Profession. 1883-84. London: Henry Kimpton.

A Practical Introduction to Medical Electricity. By A. de Watteville. Second Edition. With 89 illustrations and plates of the motor points. London: H. K. Lewis. 1884.

The Sanitary Record and London Medical Record Diary for 1884. London: Smith, Elder & Co.

Handbook of Geographical and Historical Pathology. By Dr. August Hirsch, Berlin. Translated from the Second German Edition by Charles Creighton, M.D. Vol. I.—Acute Infective Diseases. London: The New Sydenham Society. 1883.

A Text Book of the Principles of Physics. By Alfred Daniell, M.A., Lecturer on Physics in the School of Medicine, Edinburgh. London: Macmillan & Co. 1884.

A Manual of Physiology for the Use of Junior Students of Medicine. By Gerald F. Yeo, M.D. London: J. & A. Churchill. 1884.

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Prostitution Under the Regulation System, French and English. By Yves Guyot. Translated from the French by E. B. Truman, M.D. London: George Redway. 1884.

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